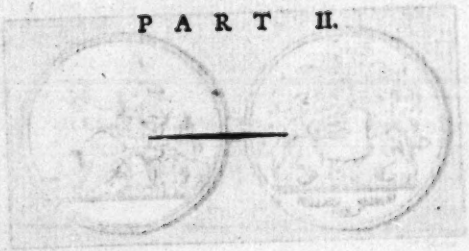
PHILOSOPHICAL TRANSACTIONS.

PART II.



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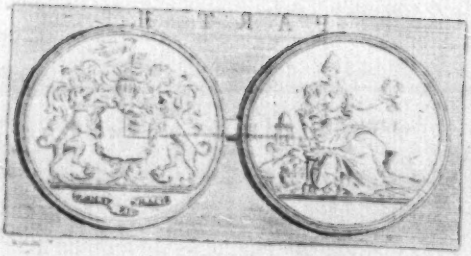
PHILOSOPHICAL TRANSACTIONS



PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXVII. For the Year 1787.

PART II.

LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
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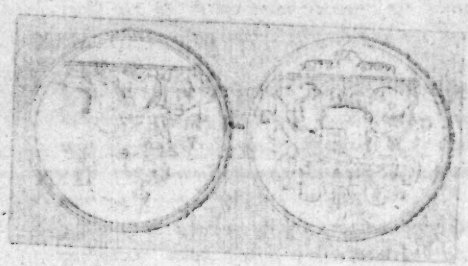

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PHILOSOPHICAL TRANSACTIONS.

XXI. *An Experiment to determine the Effect of extirpating one Ovarium upon the Number of Young produced.* By John Hunter, *Esq. F. R. S.*

Read March 22, 1787.

IN all animals of distinct sex, the females, those of the bird kind excepted, have, I believe, two ovaria, and of course the oviducts are in pairs.

By distinct sex I mean when the parts destined to the purposes of generation are of two kinds, each kind appropriated to an individual of each species, distinguished by the

appellation of male and female, and equally neceſſary to the propagation of the animal; the teſticles, with their appendages, conſtituting the male; the ovaria, and their appendages, the female ſex.

As the ovaria are the organs which on the part of the female, furniſh what is neceſſary towards the production of the third, or young animal; and as females appear to have a limited portion of the middle ſtage of life allotted for that purpoſe; it becomes a queſtion, whether thoſe organs are worn out by repeated acts of propagation; or whether there is not a natural and conſtitutional period to that power on their part, even if ſuch power has never been exerted? If we conſider this ſubject in every view, taking the human ſpecies as an example, we ſhall diſcover that circumſtances, either local or conſtitutional, may be capable of extinguiſhing in the female the faculty of propagation. Thus we may obſerve when a woman begins to breed at an early period, as at fifteen, and has her children faſt, that ſhe ſeldom breeds longer than the age of thirty or thirty-five; therefore we may ſuppoſe, either that the parts are then worn out, or that the breeding conſtitution is over. If a woman begins later, as at twenty or twenty-five, ſhe may continue to breed to the age of forty or more; and there are, now and then, inſtances of women, who, not having conceived before, have had children as late in life as at fifty years or upwards. After that, few women breed, even if they ſhould not have bred before; therefore, there muſt be a natural period to the power of conception. A ſimilar ſtop to propagation may likewiſe take place in many other claſſes of animals, probably in the female of every claſs, the period varying according to circumſtances; but ſtill we are not ena-

bled to determine, how far it depends on any particular property of the conſtitution, or of the ovarium alone.

As the female of moſt claſſes of animals has two ovaria, I imagined, that by removing one it might be poſſible to determine how far their actions were reciprocally influenced by each other, from the changes which by compariſon might be obſerved to take place, either by the breeding period being ſhortened, or perhaps, in thoſe animals whoſe nature it is to bring forth more than one at a time, by the number produced at each birth being diminiſhed.

There are two views in which this ſubject may be conſidered. The firſt, that the ovarium, when properly employed, may be a body determined and unalterable reſpecting the number of young to be produced. In that caſe we can readily imagine, that, when one ovarium is removed, the other may produce its determined number in two different ways; one when the remaining ovarium, not influenced by the loſs of the other, will produce its allotted number, and in the ſame time; the other, when it is affected by the loſs, yet the conſtitution demands the ſame number of young each time of breeding, as if there were ſtill two ovaria; conſequently it furniſhes double the number it would have been required to ſupply, had both been allowed to remain, but muſt ceaſe from the performance of its function in half the time. The ſecond view of the ſubject is by ſuppoſing, that there is not originally any fixed number which the ovarium muſt produce; but that the number is increaſed or diminiſhed according to circumſtances; that it is rather the conſtitution at large that determines the number; and that, if one ovarium is removed, the other will be called upon by the conſtitution to perform the operations of both; by which means the animal

ſhould produce, with one ovarium, the ſame number of young as would have been produced if both had remained.

With an intention to aſcertain thoſe points, as far as I could, I was led to make the following experiment; and for that purpoſe choſe pigs in preference to any other animal, becauſe they are eaſily managed, and breed perfectly well under the confinement neceſſary for experiments. Having ſelected two female ones of the ſame colour and ſize, and likewiſe a boar pig, all of the ſame farrow; after having removed one ovarium only from one of the females, and cut a ſlit in one of its ears, that there might be no miſtake between it and the other, I had them well fed and kept warm, that there might be no impediment to their breeding; and whenever they farrowed, their pigs were taken away exactly at the ſame age.

About the beginning of the year 1779, they both took the boar; but the one which had been ſpayed earlier than the perfect female. The diſtance of time, however, was not great, and they continued breeding at nearly the ſame times. The ſpayed animal continued to breed till September 1783, when ſhe was ſix years old, which was a ſpace of more than four years. In that time ſhe had eight farrows; but did not take the boar afterwards, and had in all ſeventy-ſix pigs. The perfect one continued breeding till December 1785, when ſhe was about eight years old, a period of almoſt ſix years, in which time ſhe had thirteen farrows, and had in all one hundred and ſixty two pigs; after this time ſhe did not breed: I kept her till November 1786.

I have here annexed a table of the different times of each farrow, with the number of pigs produced.

Spayed

Spayed Sow.

Farrows.	Number of young.	Time.
1.	6	Dec. 1779
2.	8	July 1780
3.	6	Jan. 1781
4.	10	Aug. 1781
5.	10	Mar. 1782
6.	9	Sept. 1782
7.	14	May 1783
8.	13	Sept. 1783
	76	

November following ſhe was put to the boar, but brought no pigs. April 1784, ſhe was again put to the boar, without effect, and never was obſerved to take the boar afterwards, although often with him. November 1784, ſhe was killed.

Perfect Sow.

Farrows.	Number of young.	Time.
1.	9	
2.	6	
3.	8	
4.	13	Dec. 1781.
5.	10	June 1782.
6.	16	Dec. 1782.
7.	13	June 1783.
8.	12	Oct. 1783.
	87	

Eleven pigs more than were produced by the ſpayed ſow in her eight farrows.

Farrows.

Farrows.	Number of young.	Time.
9	12	Feb. 1784
10	16	June 1784
11	12	Dec. 1784
12	16	May 1785
13	19	Dec. 1785
	75	

After which she bred no more.

The firſt eight farrows were	87
The laſt five farrows were	75
Total	162
The number from the ſpayed one	76
More than farrowed by the imperfect animal	86

It is obſervable, that both ſows rather increaſed in their number each time the older they grew, although not uniformly; the difference between the firſt and laſt in both animals being conſiderable.

From the above table we find, that the ſow with only one ovarium bred till ſhe was ſix years old, from the latter end of 1779 till September 1783, about four years, and in that time brought forth ſeventy-ſix pigs. The perfect animal bred till ſhe was eight years of age. In the laſt, if conception depended on the ovaria, it was to be expected, that ſhe would bring forth double the number at each birth; or, if ſhe did not, that ſhe would continue breeding for double the time. We indeed find her producing ten more than double the number of the imperfect animal, and continuing to breed much longer.

From

From a circumſtance mentioned in the courſe of this experiment it appears, that the deſire for the male continues after the power of breeding is exhauſted in the female; and therefore does not altogether depend on the powers of the ovaria to propagate, although we muſt at the ſame time allow, that it may be influenced by the exiſtence of ſuch parts.

If theſe obſervations ſhould be conſidered as depending on a ſingle experiment, from which alone it is not juſtifiable to draw concluſions, I have only to add, that the difference in the number of pigs produced by each was greater than can be juſtly imputed to accident, and is a circumſtance certainly in favour of the univerſality of the principle I wiſhed to aſcertain *.

From this experiment it ſeems moſt probable, that the ovaria are from the beginning deſtined to produce a fixed number, beyond which they cannot go, although circumſtances may tend to diminiſh that number; that the conſtitution at large has no power of giving to one ovarium the power of propagating equal to two; for, in the preſent experiment, the animal with one ovarium produced ten pigs leſs than half the number brought forth by the pig with both ovaria. But that the conſtitution has ſo far a power of influencing one ovarium, as to make it produce its number in a leſs time than would probably have been the caſe if both ovaria had been preſerved, is evident from the above recited experiment.

* It may be thought by ſome, that I ſhould have repeated this experiment; but an annual expence of twenty pounds for ten years, and the neceſſary attention to make the experiment complete, will be a ſufficient reaſon for my not having done it.

XXII. *Experiments made to determine the positive and relative Quantities of Moisture absorbed from the Atmosphere by various Substances, under similar Circumstances. By Sir* Benjamin Thompson, *Knt. F. R. S.; communicated by* Charles Blagden, *M. D. Sec. R. S.*

Read March 22, 1787.

BEING engaged in a course of experiments, upon the conducting powers of various bodies with respect to heat, and particularly of such substances as are commonly made use of for cloathing, in order to see if I could discover any relation between the conducting powers of those substances, and their power of absorbing moisture from the atmosphere, I made the following experiments.

Having provided a quantity of each of the under mentioned substances, in a state of the most perfect cleanness and purity, I exposed them, spread out upon clean China-plates, twenty-four hours in the dry air of a very warm room (which had been heated every day for several months by a German stove), the last six hours the heat being kept up to 85° of FAHRENHEIT's thermometer; after which I entered the room with a very accurate balance, and weighed equal quantities of these various substances, as expressed in the following table.

This being done, and each substance being equally spread out upon a very clean China plate, they were removed into a very large uninhabited room upon the second floor, where they were

were exposed 48 hours, upon a table placed in the middle of the room, the air of the room being at the temperature of 45° F.; after which they were carefully weighed (in the room) and were found to weigh as under mentioned.

They were then removed into a very damp cellar, and placed upon a table, in the middle of a vault, where the air, which appeared by the hygrometer to be completely saturated with moisture, was at the temperature of 45° F.; and in this situation they were suffered to remain three days and three nights, the vault being hung round, during all this time, with wet linen cloths, to render the air as damp as possible, and the door of the vault being shut.

At the end of the three days I entered the vault, with the balance, and weighed the various substances upon the spot, when they were found to weigh as is expressed in the third column of the following table.

The various substances.	Weight after being dried 24 hours in a hot room.	Weight after being exposed 48 hours in a cold, uninhabited room.	Weight after being exposed 72 hours in a damp cellar.
	Pts.	Pts.	Pts.
Sheep's wool	1000	1084	1163
Beaver's fur	1000	1072	1125
The fur of a Russian hare	1000	1065	1115
Eider down	1000	1067	1112
Silk, Raw, single thread	1000	1057	1107
Silk, Ravelings of white taffety	1000	1054	1103
Linen, Fine lint	1000	1046	1102
Linen, Ravelings of fine linen	1000	1044	1082
Cotton wool	1000	1043	1089
Silver wire, very fine, gilt, and flatted, being the ravelings of gold lace	1000	1000	1000

N. B. The weight made use of in these experiments was that of Cologne, the *parts* or least divisions being = $\frac{1}{1,1,1}$ part of a mark, consequently 1000 of these *parts* make about 521 *grains Troy*.

I did not add the silver wire to the bodies above mentioned from any idea that that substance could possibly imbibe moisture from the atmosphere; but I was willing to see whether a metal, placed in air saturated with water, is not capable of receiving a small addition of weight from the moisture attracted by it, and attached to its surface; from the result of the experiment, however, it should seem that no such attraction subsists between the metal I made use of, and the watery vapour dissolved in air.

I was totally mistaken in my conjectures relative to the results of the experiments with the other substances. As linen is known to attract water with so much avidity; and as, on the contrary, wool, hair, feathers, and other like animal substances, are made wet with so much difficulty, I had little doubt but that linen would be found to attract moisture from the atmosphere with much greater force than any of those substances; and that, under similar circumstances, it would be found to contain much more water: and I was much confirmed in this opinion upon recollecting the great difference in the apparent dampness of linen and of woollen clothes, when they are both exposed to the same atmosphere. But these experiments have convinced me, that all my speculations were founded upon erroneous principles.

It should seem, that those bodies which are the most easily wet, or which receive water, in its unelastic form, with the greatest ease, are not those which in all cases attract the watery vapour dissolved in the air with the greatest force.

Perhaps

Perhaps the apparent dampness of linen, to the touch, arises more from the ease with which that substance parts with the water it contains, than from the quantity of water it actually holds: in the same manner as a body appears hot to the touch, in consequence of its parting freely with its heat, while another body, which is actually at the same temperature, but which witholds its heat with greater obstinacy, affects the sense of feeling much less violently.

It is well known, that woollen clothes, such as flannels, &c. worn next the skin, greatly promote insensible perspiration. May not this arise principally from the strong attraction which subsists between wool and the watery vapour which is continually issuing from the human body?

That it does not depend entirely upon the warmth of that covering, is clear; for the same degree of warmth, produced by wearing more cloathing of a different kind, does not produce the same effect.

The perspiration of the human body being absorbed by a covering of flannel, it is immediately distributed through the whole thickness of that substance, and by that means exposed by a very large surface to be carried off by the atmosphere; and the loss of this watery vapour, which the flannel sustains on the one side, by evaporation, being immediately restored from the other, in consequence of the strong attraction between the flannel and this vapour, the pores of the skin are disencumbered, and they are continually surrounded by a dry, warm, and salubrious atmosphere.

I am astonished, that the custom of wearing flannel next the skin should not have prevailed more universally. I am confident it would prevent a multitude of diseases; and I know of

no greater luxury than the comfortable ſenſation which ariſes from wearing it, eſpecially after one is a little accuſtomed to it.

It is a miſtaken notion, that it is too warm a cloathing for ſummer. I have worn it in the hotteſt climates, and in all ſeaſons of the year, and never found the leaſt inconvenience from it. It is the warm bath of a perſpiration confined by a linen ſhirt, wet with ſweat, which renders the ſummer heats of ſouthern climates ſo inſupportable; but flannel promotes perſpiration, and favours its evaporation; and evaporation, as is well known, produces poſitive cold.

I firſt began to wear flannel, not from any knowledge which I had of its properties, but merely upon the recommendation of a very able phyſician (Sir RICHARD JEBB); and when I began the experiments of which I have here given an account, I little thought of diſcovering the phyſical cauſe of the good effects which I had experienced from it; nor had I the moſt diſtant idea of mentioning the circumſtance. I ſhall be happy, however, if what I have ſaid, or done, upon the ſubject, ſhould induce others to make a trial of what I have ſo long experienced with the greateſt advantage, and which, I am confident, they will find to contribute greatly to health, and conſequently to all the other comforts and enjoyments of life.

I ſhall then think theſe experiments, trifling as they may appear, by far the moſt fortunate, and the moſt important ones I have ever made.

With regard to the original object of theſe experiments, the diſcovery of the relation which I thought might poſſibly ſubſiſt between the warmth of the ſubſtances in queſtion, when made uſe of as cloathing, and their powers of attracting

moisture from the atmosphere; or, in other words, between the quantities of water they contain, and their conducting powers with regard to heat; I could not find that these properties depended in any manner upon, or were in any way connected with, each other.

The result of my experiments upon the conducting powers of these substances, I reserve for a future communication.

XXIII. *The Principles and Illuſtration of an advantageous Method of arranging the Differences of Logarithms, on lines graduated for the Purpoſe of Computation.* By *Mr.* William Nicholſon; *communicated by* Sir Joſeph Banks, *Bart. P. R. S.*

Read March 29, 1787.

1. IF two geometrical ſeries of numbers, having the ſame common ratio, be placed in order with the terms oppoſite each other; the ratio, between any term in one ſeries and its oppoſite in the other, will be conſtant *.

2. And likewiſe the ratio of a term in one ſeries to any term in the other, will be the ſame as obtains between any other two terms having the ſame relative poſition and diſtance †.

3. In all ſuch pairs of geometrical ſeries, as have the ſame common ratio, the laſt mentioned property obtains, though the firſt antecedent and conſequent be taken in one pair, and the ſecond in any other pair ‡.

* Geom. series $\begin{cases} a & an & an^2 & an^3 & an^4 \\ b & bn & bn^2 & bn^3 & bn^4 \end{cases}$

Then $a : b :: an : bn :: an^2 : bn^2$, &c.

† In the foregoing ſeries $a : bn^2 :: an^2 : bn^4 :: an : bn^3$, &c.

‡ Geo. series $\begin{cases} a & an & an^2 & an^3 & an^4 \\ b & bn & bn^2 & bn^3 & bn^4 \end{cases}$

Geo. series $\begin{cases} d & dn & dn^2 & dn^3 & dn^4 \\ \frac{bd}{a} & \frac{bdn}{a} & \frac{bdn^2}{a} & \frac{bdn^3}{a} & \frac{bdn^4}{a} \end{cases}$

Then $a : bn^2 :: dn : \frac{bdn^3}{a}$, &c.

4. If

4. If the differences of the logarithms of numbers be laid in order upon an arrangement of equi-diſtant parallel right lines, in ſuch a manner as that a right line, drawn acroſs the whole, ſhall interſect it at diviſions which denote numbers in geometrical progreſſion; then, from the condition of the arrangement and the property of this logarithmic line, it follows, firſt, that every right line, ſo drawn, will, by its interſections, indicate a geometrical ſeries of numbers*; ſecondly,

* Let AB, CD, EF (Tab. X. fig. 5.) be portions of the logarithmic line, arranged according to the condition: let GH be a right line drawn acroſs, ſo as to paſs through points of diviſion e, c, a, denoting numbers in geometrical progreſſion: then will any other line IK, drawn acroſs the arrangement, alſo paſs through points f, d, b, denoting numbers in geometrical progreſſion.

Demonſtration. From one of the extreme points of interſection f in the laſt named line IK draw the right line fg, parallel to GH, and interſecting the arrangement in the points i, b; and the ratios of the numbers $e : f$, $c : i$, and $a : b$, will be equal, becauſe the intervals on the logarithmic line, or differences of the logarithms of thoſe numbers, are equal:

Or $\frac{e}{c} = \frac{f}{i}$ and $\frac{c}{a} = \frac{i}{b}$.

But $\frac{e}{c} = \frac{c}{a}$ by the condition.

Therefore $\frac{f}{i} = \frac{i}{b}$; or the numbers f, i, b, are in the ſame continued ratio as the numbers e, c, a.

Again, the point f, the line id, and the line bb, are in arithmetical progreſſion, and denote the differences of the logarithms of the numbers f and f, i and d, b and b.

The quotients of the numbers themſelves are therefore in geometrical progreſſion, that is,

$\frac{f}{f} : \frac{d}{i} : \frac{b}{b}$, or $\frac{i}{d} = \frac{db}{bi}$.

Or $\frac{i}{d} = \frac{di}{bf}$, by ſubſtituting $\frac{i}{f}$ for its equal $\frac{b}{i}$.

Whence $\frac{f}{d} = \frac{d}{b}$ or $f : d : b$. Q. E. D.

that ſuch ſeries, as are ſo indicated by parallel right lines, will have the ſame common ratio *; and, thirdly, that the ſeries thus indicated by two parallel right lines, ſuppoſed to move laterally without changing either their mutual diſtance, or parallelism to themſelves, will have, each the ſame common ratio; and in all pairs of ſeries indicated by ſuch two lines, the ratio between an antecedent on one parallel and the oppoſite term on the other, taken as a conſequent, will be conſtant †.

5. Thus far the logarithmic line has been conſidered as unlimited. If, therefore, an antecedent and conſequent be given, it will be poſſible to find both on the arrangement, and to draw two parallel lines, one over each number: and if the lines be then ſuppoſed to move, without changing either their diſtance or abſolute direction, ſo that the line, which before marked an antecedent, may now mark a new antecedent; the other (by 2. and 3.) will mark a number, at the ſame relative poſition and diſtance, which ſhall be the conſequent to this laſt antecedent after the ſame ratio.

6. Suppoſe a logarithmic line to contain no more than a ſingle range of numbers from 1 to 10, it will not be neceſſary, for the purpoſes of computation, to repeat it; for if a ſlider

* In the ſame manner as it was proved that the line *fg*, parallel to GH, paſſes through points of diviſion, denoting numbers in the ſame continued ratio as thoſe indicated by the line GH, it may alſo be ſhewn, that the line LM parallel to any other line IK, will paſs through a ſeries of numeral points, having the ſame continued ratio as the ſeries indicated by that line IK to which it is parallel.

† Becauſe the lines preſerve their parallelism to their former ſituation, they will indicate geometrical ſeries having the ſame common ratio as before; and, becauſe their diſtance meaſured on the logarithmic line remains unchanged, the differences of the logarithms of oppoſite numbers, and conſequently their ratio, will be conſtant.

3 or

or beam have two fixed points at the distance of the interval between 1 and 10, and a moveable point be made to range between these (always to indicate the antecedent); then, I say, if the consequent fixed point fall without the rule, the other fixed point will shew the division it would have fallen on, if the rule had been prolonged. This may be easily applied to the arrangement described, N° 4.

7. If the arrangement consist only of the logarithms from 1 to 10, and the parallel cross lines intersect that geometrical series whose successive ratios altogether, with that of the last to the first, make by composition the ratio $\frac{1}{10}$, the contrivance, N° 6, may be applied to shew such consequents as fall, laterally, without the rule.

8. It is convenient that the arrangement of the lines be disposed so as to occupy a rectangular parallelogram; or, in other words, that the cross line, cutting the series last mentioned, may be at right angles to the length of the rule.

The construction of an instrument on the foregoing principles admits of various dispositions of the graduated lines and apparatus for measuring intervals upon them. Fig. 1. is a rule consisting of ten parallel lines, equivalent to a double line of numbers upwards of 20 feet in length. Fig. 2. is a beam compass for measuring intervals. The parts B, A, C, apply to the surface of the rule; the middle A being moveable sideways in a groove in the piece DE, so as always to preserve its parallelism to the external pieces B, C, which are fixed at a distance equal to the length of the rule, and have their edges placed according to the condition in § 7. which is here at right angles to the length. The piece DE, in the use, is applied to the edge FG of the rule. The edges or borders H, I, K, L, may

may be made of transparent horn or tortoise-shell, which are in many respects more convenient than any opake substance.

The Use. Apply the edge of either B or C to the consequent, and slide the piece A to the antecedent, observing the difference between the numbers on the pieces denoting the lines they are found on; then, if the same edge of A be applied to any other antecedent, the other piece B or C, made use of, will intersect a consequent in the same ratio upon that line of the arrangement, which has the same situation, with respect to the antecedent, as the line of the former consequent had to its antecedent. The numbers on the pieces serve to indicate the relative situations. But if B be the consequent piece, and fall without the rule, the piece C will shew the consequent one line lower; or if C, in the like case, fall without the rule, B will shew the consequent one line higher. It would be easy to make the same kind of provision for the numbers which fall laterally without the rule: and it might be found convenient if, for the purpose of computation, instruments of this kind were to be made with an hundred or more lines. But in the present instrument, the numbers on the pieces will answer the same purpose: for if a consequent fall on a line at any given number of intervals without the rule, it will be found on that line of the arrangement, which occupies the same number of intervals, reckoned inwards from the opposite edge of the rule.

Fig. 3. is a GUNTER's scale, equivalent to that of 28¼ inches in length, published by the late Mr. ROBERTSON. It is, however, but one-fourth of the length, and contains only one-fourth of the quantity of division. In the slider GH is a moveable piece AB, across which a fine line is drawn; and there are also lines CD, EF, drawn across the slider, at a distance from each other equal to the length of the rule. The use of this is similar to that of the foregoing. The

line

line CD or EF is to be placed at the conſequent, and the line in the piece AB at the antecedent: then, if the piece AB be placed at any other antecedent, the ſame line CD or EF will indicate its conſequent in the ſame ratio taken the ſame way; that is to ſay, if the antecedent and the conſequent lie on the ſame ſide of the ſlider, all other antecedents and conſequents in that ratio will lie in the ſame manner; and the contrary if they do not, &c. But if the conſequent line fall without the rule, the other fixed line on the ſlider will ſhew the conſequent; but on the contrary ſide of the ſlider to that where it would elſe have been ſeen by means of the firſt conſequent line.

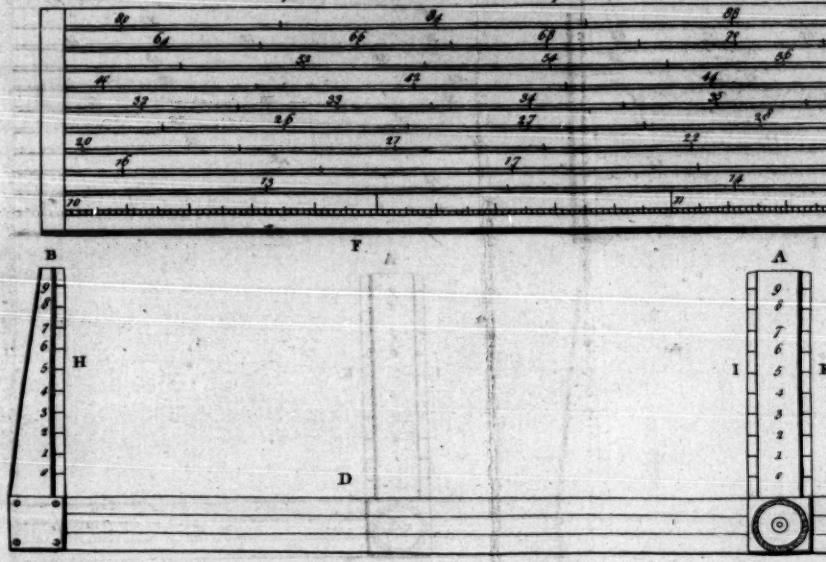
Fig. 4. is an inſtrument equivalent to the ſame rule of 28½ inches long. It conſiſts of three concentric circles engraved and graduated on a plate of about 2½ inch in diameter. From the center proceed two legs A, B, having right-lined edges in the direction of radii. They are moveable either ſingly or together. To uſe this inſtrument, place one of the edges at the antecedent, and the other at the conſequent, and fix them to that angle. The two legs being then moved together, and the antecedent leg placed at any other number, the other leg will give its conſequent in the like poſition or ſituation on the lines. If the line CD happen to lie between the legs, and B be the conſequent leg, the number ſought will be found one line farther from the center than it would otherwiſe have been; and, on the contrary, it will be found one line nearer in the like caſe, if A be the conſequent leg. This inſtrument, differing from fig. 1. only in its circular form, and the advantages reſulting from that form, the lines muſt be taken to ſucceed each other in the ſame manner laterally; ſo that numbers, which fall either without or within the arrangement of circles, will

be found on ſuch lines of the arrangement as would have occupied the vacant places if the ſucceſſion of lines had been indefinitely repeated ſideways.

I approve of this conſtruction, as ſuperior to every other which has yet occurred to me, not only in point of convenience, but likewiſe in the probability of being better executed, becauſe ſmall arcs may be graduated with very great accuracy, by diviſions transferred from a larger original. The inſtrument, fig. 1. may be conveniently contained in a circle of about 4½ inches diameter.

The circular inſtrument is a combination of the GUNTER's line and the ſector, with the improvements here pointed out. The property of the ſector may be uſeful in magnifying the differences of the logarithms in the upper part of the line of ſines, the middle of the tangents, or the beginning of the verſed ſines. It is even poſſible, as mathematicians will eaſily conceive, to draw ſpirals on which graduations of parts, every where equal to each other, will ſhew the ratios of thoſe lines by means of moveable radii ſimilar to thoſe in this inſtrument. But I do not, in this Diſcourſe, propoſe to enter into enquiries reſpecting the nature of ſuch curves, nor their utility in the preſent buſineſs.

Sketch of an Instrument, equivalent to a comm



Gunter's Scale, equivalent to those commonly made of 28½ Inches long.

mmon Gunter's Scale 20 feet in length. Fig. 1.

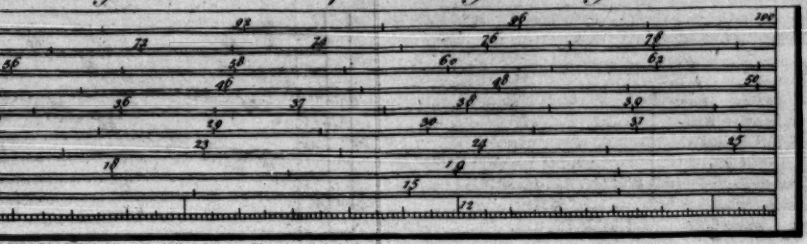
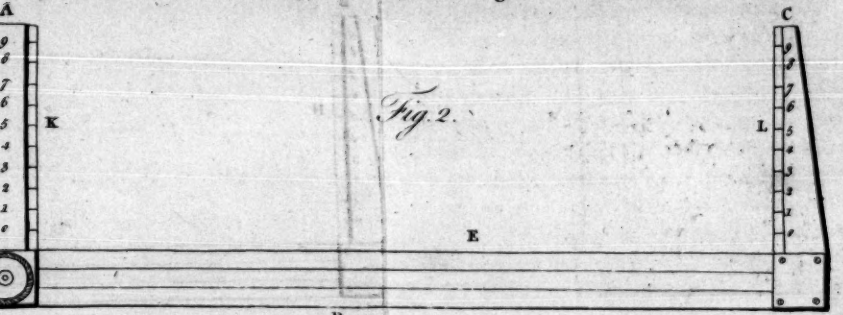
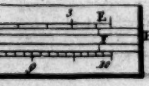
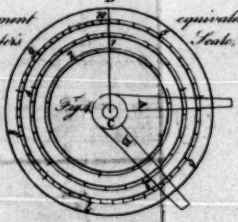



Fig. 2.

Fig. 3.



Instrument equivalent to the Gunter's Scale, Fig. 3.



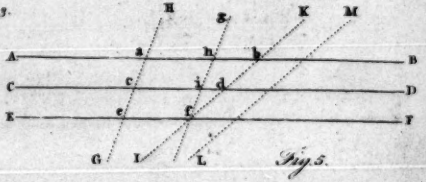


Fig. 5.

XXIV. *Observations tending to shew that the* Wolf, Jackal, *and* Dog, *are all of the same Species.* By John Hunter, *Esq.* F. R. S.

Read April 26, 1787.

THE true distinction between different species of animals must ultimately, as appears to me, be gathered from their incapacity of propagating with each other an offspring capable again of continuing itself by subsequent propagations: thus the Horse and Ass beget a Mule, capable of copulation, but incapable of begetting or producing offspring. If it be true, that the mule has been known to breed, which must be allowed to be an extraordinary fact, it will by no means be sufficient to determine the Horse and Ass to be of the same species; indeed, from the copulation of Mules being very frequent, and the circumstance of their breeding very rare, I should rather attribute it to a degree of monstrosity in the organs of the Mule which conceived, not being those of a mixed animal, but those of the Mare or female Ass. This is not so far-fetched an idea, when we consider that some true species produce monsters, which are a mixture of both sexes, and that many animals of distinct sex are incapable of breeding at all.

If then we find nature in its greatest perfection deviating from general principles, why may not it happen likewise in the production of Mules, so that sometimes a Mule shall breed from the circumstance of its being a monster respecting mules?

The times of uterine geſtation being the ſame in all the varieties of every ſpecies of animals, this circumſtance becomes neceſſary to determine a ſpecies.

The affinity between the Fox, Wolf, Jackal, and ſeveral varieties of the Dog, in their external form and ſeveral of their properties, is ſo ſtriking, that they appear to be only varieties of the ſame ſpecies. The Fox would ſeem to be a greater remove from the Dog than either the Jackal or Wolf, at leaſt in diſpoſition, not being either ſo ſociable reſpecting its own ſpecies or man, but naturally a ſolitary animal; from all which I ſhould ſuſpect it is only allied to the Dog by being of the ſame genus. It is confidently aſſerted by many, that the Fox breeds with the Dog, but this has not been accurately aſcertained; but, if it had, it would probably have been carried further, and once breeding, according to what we have ſaid, does not conſtitute a ſpecies; this, however, is a part I mean to inveſtigate. Wolves and Jackals are found in herds; and the Jackal is ſo little afraid of the human ſpecies, that, like a Dog, it comes into houſes in ſearch of food, more like a variety of the Dog in conſequence of cultivation than chance. It is by much the moſt familiar of the two; for we ſhall find hereafter, that in its readineſs to copulate with the Dog, and its familiarity with the Dog afterwards, it is ſomewhat different from the Wolf. The Wolf then being an animal better known in Europe, where inquiries of this kind are made, ſome pains has been taken to aſcertain, whether or not it was of the ſame ſpecies with the dog; but, I believe, it has been hitherto conſidered as only belonging to the ſame genus.

Accident often does as much for natural hiſtory as premeditated plans, eſpecially when nature is left to itſelf. The firſt inſtance of the Dog and Wolf breeding in this country

ſeems

ſeems to have been about the year 1766. A Pomeranian Bitch of Mr. BROOKES's, in the New Road, was lined only once by a Wolf, and brought forth a litter of nine healthy puppies. The veracity of Mr. BROOKES is not to be doubted, reſpecting the Bitch being lined by a Wolf; yet, as it was poſſible ſhe might have been lined by ſome common Dog without his knowledge, the fact was not clearly made out; but it has been ſince aſcertained, that the Dog and Wolf will breed. Several noblemen and gentlemen bought ſome of the puppies, as I was informed by Mr. BROOKES. My Lord CLANBRASSIL purchaſed a Bitch-puppy; and Mr. BROOKES preſented one to me, which I kept for obſervations and experiment. Its actions were not truly thoſe of a Dog; it had more quickneſs in attending to things, was more eaſily ſtartled, as if particularly apprehenſive of danger, quicker in tranſitions from one action to another, not ſo ready to the call, being leſs docile; and from theſe peculiarities it loſt its life, being ſtoned to death in the ſtreets for a mad dog.

Hearing that Lord CLANBRASSIL's Bitch had bred, Sir JOSEPH BANKS was ſo obliging as, at my requeſt, to write to his Lordſhip, who ſent the following account.

SIR,

About ſeventeen or eighteen years ago, the late Lord MONTHERMER and I happened to ſee a Dog-wolf at Mr. BROOKES's, who deals in animals, and lives in the New Road. The animal was remarkably tame; and it ſtruck us, for that reaſon, that a breed might be procured between him and a Bitch.

We promiſed Mr. BROOKES a good price for puppies, if he ſucceeded. In about a year a Bitch produced nine, and Lord

MONTHERMER bought one; and I had another, which was a Bitch. Lord MONTHERMER's died of fits in about two years: mine lived longer, and had puppies only once. One I gave to Lord PEMBROKE; but what became of it I do not remember. It was grand-daughter of the Wolf by the dam, and got by a large Pointer of mine.

It might be conſidered, that Mr. BROOKES's word was not ſufficient proof that the puppies were really got by the Wolf; but the appearance of the animals, ſo totally different from all others of the canine ſpecies, did not leave a doubt upon our minds; and I remember HANS STANLEY, who had adopted BUFFON's opinion, was thoroughly convinced upon ſeeing mine. The animals had the ſhape of the Wolf refined: the fur long, but almoſt as fine as that of the black Fox.

I am afraid I have treſpaſſed too much upon your time, and will only beg you will be aſſured nothing can give me more pleaſure than any opportunity of aſſuring you how truly

I am, Sir, &c.

CLANBRASSIL.

Jan. 7, 1787.

Upon the ſuppoſition that Mr. BROOKES's Bitch was lined by no Dog but the Wolf, which I think we have no reaſon to doubt, the ſpecies of the Wolf is aſcertained; but I choſe to trace this breed ſtill further; and hearing that Lord PEMBROKE's Bitch had likewiſe bred, I was anxious to know the truth of it; and, finding his lordſhip was in France, I took the liberty of writing to Lord HERBERT, and received in anſwer the following letter.

SIR,

SIR, Wilton-house, Dec. 20, 1786.

The half-bred Wolf-bitch you allude to was given, as I have always understood, to Lord PEMBROKE by Lord CLANBRASSIL. She might, perhaps, have been bought at BROOKES's by him. She had four litters, one of ten puppies, by a Dog between a Mastiff and a Bull-dog. One of these was given to Dr. EYRE, at Wells in Somersetshire, and one to Mr. BUCKETT, at Stockbridge. The second litter was of nine puppies, some of which were sent to Ireland, but to whom I know not. This litter was by a different Dog, but of the same breed as the first. The third litter was of eight puppies, by a large Mastiff. Two of these were, I believe, sent to the present Duke of QUEENSBERRY. The fourth litter consisted of seven puppies; two of which were sent to M. CERJAT, a gentleman who now resides at Lausanne in Switzerland, and is famous for breaking dogs remarkably well. These two puppies were, however, naturally so wild and unruly, that he found it impossible to break them. She died four years ago, and the following inscription was put over the place where she is buried in this garden, by Lord PEMBROKE's orders.

Here lies Lupa,
whose grand-mother was a Wolf,
whose father and grand-father were Dogs, and whose
mother was half Wolf and half Dog. She died
on the 16th of October, 1782, aged 12 years.

I am sorry it is not in my power to give you any better account; but if you think proper to write to Lord PEMBROKE,

who is at Paris, I am convinced he will be very happy to give you any further information.

I am, &c.

HERBERT.

BUFFON, whoſe remarks in natural hiſtory are well known, made experiments to aſcertain how far the Wolf and Dog were of the ſame ſpecies, but without ſucceſs. He ſays, "A She-"wolf, which I kept three years, although ſhut up very "young, and along with a Greyhound of the ſame age, in a "ſpacious yard, could not be brought to agree with it, nor "endure it, even when ſhe was in heat. She was the weakeſt, "yet the moſt miſchievous; provoking, attacking, and biting "the Dog, which at firſt only defended itſelf, but at laſt "killed her." And in another part of his work, he makes the following obſervation: "The Dog, the Wolf, the Fox, "and the Jackal, form a genus, of which the different ſpe-"cies are really ſo nearly allied to each other, and of which "the individuals reſemble each other ſo much, particularly by "the internal ſtructure and parts of generation, that it is diffi-"cult to conceive why they do not breed together *."

This

* In the Supplement to his Works, he gives the following account which had been ſent to him. "A very young She-wolf, brought up at the Marquis of SPON-"TIN's, at Namur, had a Dog, of nearly the ſame age, kept with it as a "companion. For two years they were at liberty, coming and going about the "apartments, the kitchen, the ſtables, &c. lying under the table, and upon the "feet of thoſe who ſat round it. They lived in the greateſt familiarity.

"The Dog was a ſtrong Greyhound. The wolf was fed on milk for ſix months; "after that, raw meat was given her, which ſhe preferred to that which was "dreſſed.

This part of natural hiſtory lay dormant till Mr. GOUGH, who ſells birds and has a collection of animals on Holborn Hill, repeated the experiment on a Wolf-bitch, which was very tame, and had all the actions of a Dog under confinement. A Dog is the moſt proper ſubject for compariſon, as we have opportunities of being acquainted with its diſpoſitions and modes of expreſſing its ſenſations, which are moſt diſtinguiſhable in the motion of the ears and tail; ſuch as pricking up the ears when anxious, wiſhing, or in expectation; depreſſing them when ſupplicant, or in fear; raiſing the tail in anger or love, depreſſing it in fear, and moving it laterally in friendſhip; and likewiſe in raiſing the hair on the back from many affections of the mind. This animal became in heat in the month of December 1785; and as Mr. GOUGH had ſome idea of breeding from wild animals, as Monkies, Leopards, &c. he was anxious to have the Wolf lined by ſome Dog; but ſhe

" dreſſed. When ſhe ate no one durſt approach her; but at other times people " might do as they pleaſed, provided they did not uſe her ill. At firſt ſhe made " much of all the Dogs which were brought to her; but afterward ſhe gave the " preference to her old companion, and from that time ſhe became very fierce " if any ſtrange Dog approached her. She was lined for the firſt time on the " 25th of March; this was frequently repeated while her heat continued, which " was ſixteen days; and ſhe littered the 6th of June, at eight o'clock in the " morning; the period of geſtation was therefore ſeventy three days at the " moſt *. She brought forth four young ones of a blackiſh colour, ſome of " whoſe feet, and a part of the breaſt, were white; in this reſpect taking after " the Dog, who was black and white. From the time ſhe littered ſhe became " ſurly, and ſet up her back at thoſe who came near her; did not know her " maſters, and would even have killed the Dog, if it had been in her power."

* This is a longer period than in the Bitch by at leaſt ten days; but as the account was made from the firſt time of her being lined, and ſhe was in heat for a fortnight, and lined in that time, it is very probable, if the time was known when ſhe conceived, that it would prove to be the ſame period as in the Dog.

would

would not allow any Dog to come near her, probably from her not being accuſtomed to be with Dogs, and being always chained. She was held, however, while a greyhound Dog lined her, and they were faſtened together exactly as the Dog and Bitch. While in conjunction ſhe was pretty quiet; but when at liberty, ſhe endeavoured to fly at the Dog. In this way ſhe was twice lined. She conceived, and brought forth four young ones. The time ſhe went with young was not exactly known; but it was believed to be the ſame as in the Bitch. Two of the puppies were like the Dog in colour, who had large black ſpots on a white ground; one was of a black colour, and the fourth of a kind of dun, and would probably have been like the mother. She took great care of them, yet did not ſeem very anxious when one was taken from her by the keeper; nor did ſhe ſeem afraid when ſtrangers came into the room. Unfortunately theſe experiments were carried no further; one being ſold to a gentleman, who carried it to the Eaſt-Indies; and the other three were killed by a Leopard, one of which I was to have had. The ſame Wolf was in heat in December 1786, and was lined ſeveral times by a Dog. She pupped on the 24th of February 1787, and had ſix puppies, which may afford opportunities, if they are thought neceſſary, of repeating experiments on this ſubject.

While purſuing this ſubject, I was informed, that Captain MEARS, of the Royal Biſhop Eaſt-India-man, had brought home a bitch Jackal with young, which had brought forth ſoon after his arrival; and that he had given the bitch Jackal and one puppy to Mr. BAILEY, Bird-merchant, in Piccadilly. I went to ſee them, and purchaſed the puppy, the ſubject of the following experiment, which had diſpoſitions very ſimilar

to

to the half-bred Wolf which I had from Mr. BROOKES before mentioned.

To have a true hiſtory of this animal, I took the liberty of writing to Mr. MEARS, who politely called upon me, and, at my requeſt, put down the particulars in the form of a letter to me, of which the following is a copy.

SIR,

I had the honour of yours the 15th inſtant; and with regard to the female Jackal, I can aſſure you, that ſhe took a ſmall ſpaniel Dog of mine on board my ſhip, the Royal Biſhop. I had her, when a cub, at Bombay; and a very ſhort time before I arrived in England ſhe got to heat, and enticed this ſmall dog into the long-boat, where I ſaw them repeatedly faſt together. I brought her to my houſe in the country, where ſhe pupped ſix puppies, one of which you have ſeen. Mr. PLAW, at N° 90, Tottenham-Court-Road, has a Dog-puppy, which will be at your ſervice at any time you chuſe to ſend for him, to make any further experiments: I called on Mr. PLAW, and got his promiſe to let you have the Dog.

I have the honour to be, Sir, &c.

WM. MEARS.

N° 107, Hatton-ſtreet,
16th Jan. 1786.

P. S. I had the Bitch on board fourteen months.

I took this puppy into the country, and chained it up near a maſtiff Dog, and they were very familiar, and ſeemingly fond of each other. When the Bitch became firſt in heat, I could not get a proper Dog for her; but the latter end of September being again in the ſame ſituation, ſeveral Dogs were procured, and left with her. They appeared indifferent about her; probably from being in a ſtrange place; and ſhe did not ſeem inclined to be familiar with them; whether the great Dog might be able to line her I do not know; ſhe was, however, twice tied by a Tarrier on the 3d of October. In a few weeks ſhe was evidently become bigger; and on the 30th of November, in all fifty-nine days, ſhe brought forth five puppies. Some days before this period ſhe dug a hole under ground, by the ſide of her kennel, in which ſhe brought forth, and it was ſome time before ſhe would allow the puppies to ſtay in the kennel when put there. In about eight days ſome, and nine days others of them began to open their eyelids.

Here then is an abſolute proof of the Jackal being a Dog; and it appears to me, that the Wolf is equally made out to be of the ſame ſpecies. It now then becomes a queſtion, whether the Wolf is from the Jackal, or the Jackal from the Wolf, ſuppoſing they had but one origin? From the ſuppoſition, that varieties become more tame in their nature, we ſhould be led to believe, the Wolf to be the original, and that the Jackal was a ſtep towards civiliſation in that ſpecies of animal. There are Wolves of various kinds, each country having a Wolf peculiar to itſelf; but the Jackals that I have ſeen have been more uniformly the ſame, both thoſe from Africa, and thoſe from the Eaſt-Indies. I am informed, however, that they vary in ſize. Whether all the Wolves of different countries are of one ſpecies, or ſome of them only of the

ſame

ſame genus, I do not know; but I ſhould rather ſuppoſe them to be all of one ſpecies. What is with me an argument in favour of this ſuppoſition is, that, if there were Wolves of diſtinct ſpecies, we ſhould have had by this time a great variety of that ſpecies of Wolves, with the various diſpoſitions ariſing from variation in other reſpects; and thoſe varieties now turned to very uſeful purpoſes, as has been the caſe with the Dog; for all the Wolves we are yet acquainted with, have naturally the principle of cultivation in them, as much probably as any animal, or as much at leaſt as thoſe Wolves we now know to be Dogs. The not having a civiliſed ſpecies of Wolf is, indeed, with me a proof that they are all of the ſame ſpecies with the Dog. If they are all of the ſame ſpecies with the Dog, then the firſt variety that took place was ſtill in the character of a Wolf, differing only in colour, or ſome trivial circumſtance, which could only take place from a difference in climate; civiliſation or cultivation in a ſtate of nature being the ſame in them all. Where they became Jackal, or what we now call Dog, is difficult to ſay; or what Dog we can call the firſt remove, as many Dogs differ very much from one another; or whether the Jackal is the intermediate link between the Wolf and the Dog. In either caſe we have three great varieties in this ſpecies, Wolf, Jackal, and Dog, with the varieties in each. If the Dog is proved to be the Wolf tamed, the Jackal may probably be the Dog returned to his wild ſtate.

To aſcertain the original animal of a ſpecies, it is proper to examine all the varieties of that ſpecies, and ſee how far they have the character of the genus, and what reſemblance they bear to the other ſpecies of the genus; for it is natural to ſuppoſe, that the original, or the animal which is neareſt to it,

will have more of the true character of the genus, and will have a ſtronger reſemblance to the ſpecies neareſt allied to it, than any of the other varieties of its own ſpecies.

If we apply this to the Dog, and conſider the Fox as a diſtinct ſpecies, which there is great reaſon to believe it is, that variety which has the ſtrongeſt reſemblance to the Fox, is to be looked upon as the original of all the others; which will prove to be the Wolf.

Another mode of conſidering this ſubject, which is however ſecondary to the above, is, ſuppoſing that all animals were at firſt wild; and, therefore, that thoſe animals which remain wild, are the original ſtock; and that the further we find animals removed from their originals in appearance, they are really further removed in conſequence of variation taking place from cultivation, ſo that we may ſtill be able to trace the gradation. What gives ſome force to this idea is, that where the Dogs have been leaſt cultivated, there they ſtill retain moſt of their original character, or ſimilarity to the Wolf or the Jackal, both in ſhape and diſpoſition. Thus the ſhepherd's Dog, all over the world, has ſtrongly the character of the Wolf or Jackal; ſo that but little difference is to be obſerved, except in ſize and hair. Size is, perhaps, a variety taking place under a variety of circumſtances; but difference in hair is, in general, influenced by climate, although perhaps not always ſo. Thus the Wolf has longer and ſofter hair than the Jackal, becauſe he is a more northern animal; and the Jackal and ſhepherd's Dog in Portugal and Spain have ſhorter and ſtronger hair than thoſe of Germany or Kamchatka, from inhabiting warmer climates. But when we conſider their general ſhape, the character of countenance, the quick manner with the pricked and erect ears, we muſt ſuppoſe them varieties

of

of the ſame ſpecies. The ſmelling at the tail has been deſcribed as characteriſtic of the Dog; but, I believe, it is common to moſt animals, and only marks the male; for it is the moſt certain way the male has of knowing the female, and alſo diſcloſes another ſcent, which is the final intention, whether the female is diſpoſed to receive the male.

The Eſquimaux Dog, and that found among the Indians as far ſouth as the Cherokees; the ſhepherd's Dog in Germany, called Pomeranian; the ſhepherd's Dog in Portugal and Spain; have all a ſtrong ſimilarity to the Wolf and Jackal.

Buffon, on the origin of Dogs, ſeems to have poſſeſſed nearly the ſame idea; for he ſays the ſhepherd's Dog is the original ſtock from which the different races of Dogs have ſprung.

As the Wolf turns out to be a Dog, it ſeems aſtoniſhing, that there was no account of Dogs being found in America. But this I conſider as a defect in the firſt hiſtory of that country, for there are Wolves; and I think, in ſpite of all that has been ſaid to the contrary, the Eſquimaux and Indian Dog is only a variety from a Wolf in that country, which had been tamed. Mr. Cameron, of Titchfield-ſtreet, who was many years among the Cherokees, and conſiderably to the weſtward of that country, obſerves, that the Dog found there is very ſimilar to the Wolf; and that the natives conſider it to be a ſpecies of tame Wolf; but as we come more among the Europeans who have ſettled there, the Dogs are more of a mixed breed; for why they ſhould only have had this kind of Dog tranſported among them, while every other part of America has the varieties of Europe, is not eaſily ſolved.

The voice of animals is commonly characteriſtic of the ſpecies; but I ſhould ſuppoſe, it is only characteriſtic of the original ſpecies, and not always of the variety, and this

ſuppoſition holds good in the Dog ſpecies. It would appear, that the voice of the Wolf and the Jackal is very ſimilar, and is principally conveyed through the noſe, and exactly reſembles that noiſe in Dogs, which is a mark of longing or melancholy, and alſo of fondneſs; but has no reſemblance to the bark of the Dog, which they do not perform. Barking is peculiar to certain varieties of the Dog kind, and even ſome that do bark, do it leſs than others. The Dogs in the South-Sea iſlands do not bark: our Greyhound barks but little; while the Maſtiff, and many of the ſmaller tribe, as Spaniels, are particularly noiſy in this way. It would appear as if the frequency of this noiſe aroſe from imitation; for the Dogs in the South-Seas learn to bark; and others, as the Hound, have a peculiar howl, which, by huntſmen, is called the tongue. This noiſe, as alſo the bark, is made by opening the mouth. A variety in the voice, or ſome parts of the voice, in the varieties of the ſame ſpecies, is not peculiar to the Dog.

XXV. *Experiments on the Congelation of the Vitriolic Acid. By* James Keir, *Esq. F. R. S.*; *communicated by* Henry Cavendish, *Esq. F. R. S.*

Read May 3, 1787.

THAT the vitriolic acid sometimes assumes a solid, crystalline state, has been observed by BASIL VALENTINE, and by many later chemists; but their relations of this appearance are neither sufficiently explicit, with regard to the essential and concomitant circumstances, nor do they seem very consistent with each other. It appears, however, that two very distinct species of congelation of this acid have been noticed. That which is described by the older chemists, and also by some modern authors, requires no greater degree of cold than the common temperature of the air, even in summer, and is peculiar to that acid which is obtained by distillation from martial vitriol, and which is possessed of a smoking quality in a high degree: for not only the authors, by whom this congelation has been observed, have given this description of the acid employed, but also the late experiments of M. DOLLFUSS * seem to shew, that the smoking quality is essential to the phænomenon; for neither the acid obtained from vitriol, when deprived by rectification of its smoking quality, nor the English oil of vitriol (which is known to be obtained by burning sulphur, and

* Crell Annalen 1785.

which

which does not ſmoke), were found, by his trials, to be ſuſceptible of this ſpecies of congelation. The acid, thus congealed, has been called *glacial*, or *icy oil of vitriol*.

The other kind of congelation has been little noticed till lately. To this congelation every kind of vitriolic acid is ſubject, whether it ſmokes or not, and whether it has been prepared from martial vitriol, or from ſulphur, provided the cold to which it is expoſed be ſufficiently intenſe: for the cold, requiſite for this ſpecies of congelation, is conſiderably greater than what is ſufficient for the former.

Mr. MACQUER relates, in the ſecond edition of his Dictionary of Chemiſtry *, that the Duke D'AYEN had obſerved the congelation of concentrated vitriolic acid, which had been expoſed to a cold expreſſed by 13 or 14 degrees below 0 of REAUMUR's ſcale; but that mixtures, conſiſting of one part of the above-mentioned concentrated acid, with two or more parts of water, could not be frozen by the cold to which he expoſed them, till he had diluted the acid ſo much, that its denſity was to that of water as 1041 to 96; in which latter caſe of congelation, it is probable, that the water only did freeze, as it does in dilute ſolutions of neutral ſalts. M. DE MORVEAU † has made ſimilar experiments, with a view to verify thoſe of the Duke D'AYEN, and with ſimilar ſucceſs. By means of an intenſe cold, produced by adding ſpirit of nitre to pounded ice, he congealed a part of ſome vitriolic acid, which he had previouſly concentrated. He obſerved, that although a very intenſe cold had been employed to freeze the concentrated acid, it nevertheleſs remained congealed in much leſs degrees of cold, and that it thawed very ſlowly. Laſtly, ſome experiments

* Article, Vitriolic Acid.

† Mem. de l'Acad. de Dijon, pour 1782.

have

have lately been made by Mr. Mc Nab, at Hudſon's Bay, on the congelation of acids by intenſe cold; an account of which experiments is given in the Philoſophical Tranſactions for 1786, by Mr. Cavendish, at whoſe deſire they had been made. Theſe experiments are the more valuable, as the denſity of the acids employed, and the temperature, and other concomitant circumſtances, have been diſtinctly noted; and they are rendered ſtill more intereſting, by the very judicious remarks made on them by Mr. Cavendish. It is there related, that a vitriolic acid, whoſe ſpecific gravity was to that of water as 1843.7 to 1000, froze when expoſed to a cold of $-15°$ of Fahrenheit's ſcale; that another more dilute vitriolic acid, conſiſting of 629 parts of the former concentrated acid, and 351 parts of water, congealed in a temperature of $-36°$; and that when the acid was further diluted, it was found capable of ſuſtaining a much greater cold without freezing. In theſe experiments, as alſo in thoſe of M. de Morveau, it appeared, that the whole of the acid did not congeal, but that part of it retained its fluidity. Mr. Cavendish found, on examining the part which had congealed, and that which had remained fluid, that they were *nearly* of the ſame ſtrength; and he is thence led to think, that the difference between them, by which the one is more diſpoſed to congeal than the other, does not depend on their different ſtrengths, but on ſome quality leſs obvious, and the ſame which conſtitutes the difference between glacial and common oil of vitriol. In all the experiments which had been made by the Duke d'Ayen, M. de Morveau, and Mr. Mc Nab, the vitriolic acid, when ſtrong, had frozen with leſs cold than when diluted; but theſe experiments did not enable Mr. Cavendish to determine, whether this acid has any determinate ſtrength or *point of eaſieſt freezing*

(ſuch as he had diſcovered to be poſſeſſed by ſpirit of nitre), or whether the cold requiſite for congelation does not continually diminiſh, as the ſtrength of the acid increaſes, without limitation. This latter opinion he thinks the moſt probable, from the circumſtance of the Duke D'AYEN's and M. DE MORVEAU's acids having frozen with a conſiderably leſs intenſe cold than thoſe of Mr. Mc NAB, which, he ſuppoſes, were weaker, as the former acids had been concentrated purpoſely.

The obſervations which I have made, and am going to relate, apply ſolely to the latter kind of congelation of the vitriolic acid, as the acid which I employed was of the kind that is prepared by burning ſulphur, and is commonly ſold in England under the name of oil of vitriol, and was perfectly free from colour, ſmell, or ſmoking quality. After a ſevere froſt at the end of the year 1784, and beginning of 1785, I obſerved that ſome vitriolic acid, contained in a corked phial, had congealed; while other parcels of the ſame acid, ſome ſtronger and ſome weaker, equally expoſed to the cold, had remained fluid. As I imputed the congelation to the great intenſity of the cold, I was afterwards much ſurpriſed, when the froſt ceaſed, to find that the acid remained frozen during many days, when the temperature of the air was ſometimes above 40° of FAHRENHEIT's ſcale; and when the congealed acid was brought into a warm room, purpoſely to thaw it, a thermometer, placed in contact with it during its thawing, continued ſtationary at 45°. From theſe circumſtances I concluded, that the freezing and thawing point of this acid was very near the laſt mentioned degree; and, accordingly, upon expoſing the liquor which had been thawed to the air, at the temperature of 30°, the congelation again took place in a few hours. From the circumſtance of other parcels of the ſame acid, but of

different ſtrengths, remaining fluid, although they had been expoſed to a much greater cold than was neceſſary for the congelation of that acid liquor which had frozen, I was led to believe, that there muſt be ſome certain ſtrength at which the vitriolic acid was more diſpoſed to freeze than at any other, greater or leſs. I knew that the ſpecific gravity of the acid which had frozen was nearly to that of water as 1800 to 1000, and that of the ſtronger acid, which had not frozen, was as 1846 to 1000; which laſt is the uſual denſity of the oil of vitriol commonly ſold in England. I knew alſo, that the acid which had frozen was in no reſpect but in ſtrength different from the ſtronger acid which had retained its fluidity; having myſelf, ſome weeks before, taken the former acid from the bottle containing the latter, and diluted it with water till it was reduced to the ſpecific gravity of 1800.

Although from the above obſervations I was convinced of the propoſition generally, that the vitriolic acid is moſt diſpoſed to freeze when at a certain ſtrength, and then it is ſuſceptible of congelation by means of much leſs cold than has been hitherto imagined; yet, as only part of my acid had frozen, I could not with certainty know the ſtrength of the frozen part, and I therefore was not able to ſtate, with any accuracy, the degree of ſtrength moſt favourable to congelation, nor the limits of ſtrength within which the acid may be congealed by ſuch moderate cold. In the following winter I had not leiſure to purſue the ſubject; but ſince the commencement of the preſent year, I have verified my former obſervation with more attention to the exact denſities of the acids; and I have found, that the point of ſtrength moſt favourable to congelation is very determinate, and that a very ſmall variation above or below that point renders the acid incapable of freezing without a conſiderable augmentation of cold. As the

acid, when brought to the proper ſtrength, was capable of freezing with leſs cold than water does, I immerſed ſeveral acids of different ſtrengths in melting ſnow, inſtead of expoſing them to the air, the temperature of which was variable, whereas that of melting ſnow was conſtant and determinate. Thoſe acids which would not freeze in melting ſnow, were afterwards immerſed in a mixture of ſnow, water, and common ſalt, the temperature of which was not ſo conſtant and determinate as that of melting ſnow; but it generally remained for ſeveral hours at about 18°, and was ſometimes ſeveral degrees lower. The intention of adding water to the ſnow and ſalt was to leſſen the intenſity of the cold of this mixture, and to render it more permanent than if the ſnow and ſalt alone were mixed.

The acids which had frozen in melting ſnow, and which were five in number, having been thawed and brought to the temperature of 60°, were found on examination to have the following ſpecific gravities.

1786
1784
1780
1778
1775

Thoſe acids which would not freeze in melting ſnow, but which froze when immerſed in ſnow, water, and ſalt, having been expoſed to a greater cold, were of a greater latitude of denſity. Their ſpecific gravities, when brought to the temperature of 60°, were found to be expreſſed by the following numbers.

1814
1810
1804

1804
1794
1790
1770
1759
1750

The acids which remained, and which would not freeze either in melting ſnow, or in the mixture of ſnow, ſalt, and water, were found on examination to have the following ſpecific gravities.

1846
1839
1815
1745
1720
1700
1610
1551

It appears, from the firſt table of ſpecific gravities, that the medium denſity of the acids which did freeze with the cold of melting ſnow was 1780; and from the ſecond table it appears, that, at the denſities of 1790 and 1770, the acid had been incapable of freezing with that degree of cold. Hence it follows, that 1780 is nearly the ſtrength or denſity of eaſieſt freezing; and that an increaſe or diminution of that denſity, equal to $\frac{1}{110}$th part, renders the acid incapable of freezing with the cold of melting ſnow, notwithſtanding this cold is ſome degrees above the freezing point of the moſt congelable acid. From the ſecond table of ſpecific gravities it appears, that by applying a more intenſe cold, namely, that produced by a mixture of ſnow, ſalt, and water, the limits of the denſity of the acids capable of congelation were extended to about $\frac{1}{111}$ above or below the point of eaſieſt freezing: and there

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ſeems little reaſon to doubt, that, by greater augmentations of cold, theſe limits may be further extended; but in what ratio theſe augmentations and extenſions proceed cannot be determined without many obſervations made in different temperatures.

Although it is probable, that the moſt concentrated acids may be frozen, provided the cold be ſufficiently intenſe, yet there ſeems to me reaſon to believe, that ſome of the congelations which have been obſerved in highly concentrated acids have been effected in conſequence of the denſity of theſe acids having been reduced nearly to the point of eaſy freezing by their having abſorbed moiſture from the air: for the Duke D'AYEN and M. DE MORVEAU expoſed their acids to the air, in cups or open veſſels; and the latter author even acquaints us, that, on examining the ſpecific gravity of the acid which had frozen, he found it to be to that of water as 129 to 74; which denſity being leſs than the point of eaſieſt freezing, proves that the acid which he employed, and which he had previouſly concentrated, had actually been weakened during the experiment. I have ſeveral times expoſed concentrated oil of vitriol in open veſſels in froſty weather; and I have ſometimes, but not always, obſerved a congelation take place. Upon ſeparating the fluid from the congealed part, and upon examining the ſpecific gravity of the latter, after it had thawed, I found that it had been reduced to the ſtandard of eaſieſt freezing. When the congealed acid was kept longer expoſed, it gradually thawed, even when the cold of the air increaſed; the reaſon of which is not to be imputed to the heat produced by the moiſture of the air mixing with the acid, for this cauſe operated during the congelation, but principally to the diminution of denſity below the point of eaſy freezing, which was occaſioned by the continued abſorption of moiſture from the air, and which

rendered

rendered the acid incapable of continuing frozen without a great increaſe of cold.

It appears then, that the concentration of M. DE MORVEAU's acid, at the time of its congelation, from which circumſtance Mr. CAVENDISH infers generally, that the vitriolic acid freezes more eaſily as it is more denſe, is not a true premiſe; and that therefore the inference, though juſtly deduced, is invalid. On the contrary, there ſeems every reaſon to believe, from the analogy of my experiments above mentioned, that as the denſity of the acid increaſes beyond the point of eaſieſt freezing, the facility of the congelation diminiſhes; at leaſt, to as great denſity as we have been ever able to obtain the vitriolic acid; for if it were poſſible to diveſt it intirely of water, it would probably aſſume a ſolid ſtate in any temperature of the air.

The cryſtallization of the frozen vitriolic acid is more or leſs diſtinct, according to the ſlowneſs of its formation, and other favourable circumſtances. Sometimes the cryſtals are very diſtinctly ſhaped, large, and very hard. Their form is the ſame as the common form of mineral alkali and of ſelenitic ſpar, but with angles different in dimenſions from either of theſe. They are ſolids conſiſting of ten faces, of which the two largeſt are equal, parallel, and oppoſite to each other, and are oblique-angled parallelograms or rhomboids, whoſe angles are, as near as I could meaſure them, of 105 and 75 degrees. Between theſe two rhomboidal faces are placed eight faces of the form of trapeziums. Thus each cryſtal may be ſuppoſed to be compoſed of two equal and ſimilar fruſtums of pyramids joined together by their rhomboidal baſes. I obſerved, that the cryſtals always ſunk in the fluid acid to the bottom of the veſſel, which ſhewed that their denſity was increaſed by congelation.

I thought

I thought of aſcertaining their ſpecific gravity by adding gradually to this fluid part ſome concentrated vitriolic acid, till the cryſtals ſhould float in the liquor, the examination of whoſe ſpecific gravity would determine that of the floating cryſtals. But I was ſurpriſed to find, that the cryſtals ſunk even in the concentrated acid, and conſequently were denſer. I then poured ſome of the congelable acid, previouſly brought to the freezing temperature, into a graduated narrow cylindrical glaſs, up to a certain mark, which indicated a ſpace equal to that occupied by 200 grains of water. The glaſs was placed in a mixture of ſnow, ſalt, and water, and when the acid was frozen, a mark was made on the part of the glaſs to which the acid had ſunk. Having thawed the acid, and emptied the glaſs, I filled it with water to the mark to which the acid had ſunk by freezing, and I found, that fifteen grains more of water were required to raiſe it to the mark expreſſing 200 grains; which ſhews, that the diminution of bulk, ſuſtained by the acid in freezing, had been equal to $\frac{1}{13.3}$ of the whole.

Computing from this *datum*, we ſhould eſtimate the ſpecific gravity of the congealed acid to have been 1924; but as it contained evidently a great number of bubbles, its real ſpecific gravity muſt be conſiderably greater than the above determination, and cannot eaſily be aſcertained on account of theſe bubbles. By way of compariſon, I obſerved the alteration of bulk which water contained in the ſame cylindrical glaſs would ſuffer by freezing; and I found that its expanſion was equal to about $\frac{1}{10}$th part of its bulk. The water had been previouſly boiled; but it nevertheleſs contained numberleſs bubbles. In this reſpect then there is a remarkable difference between the congelations of water and of vitriolic acid; but, perhaps, the

difference

difference arises principally from the bubbles of elastic fluid, which may be in greater quantity, and may add more to the bulk of the water than of the acid.

Greater cold is produced by mixing snow or pounded ice with the congealed than with the fluid acid, but the quantity I have not determined. There is reason to believe it may be considerable. In the experiments made at Hudson's Bay, by Mr. Mc NAB, the greatest cold which he had produced by mixing acids with snow, was effected by a vitriolic acid which had previously congealed; and to this circumstance of the congelation of the acid, Mr. CAVENDISH justly imputes the intensity of the cold, as the liquefaction of both the frozen acid and the snow had concurred towards this effect; whereas, in mixing fluid acids with snow, the thawing of the snow is probably the sole productive cause.

I was desirous of comparing the times required for the liquefaction of ice and of congealed acid, when both were exposed to the same temperature. For this purpose I filled two equal and similar cylindrical glasses; one with the congelable vitriolic acid, and the other with water; and, after having immersed them in a freezing mixture till both fluids were frozen, and reduced to the temperature of 28°, I withdrew the glasses from the freezing mixture, wiped them dry, and placed them together in a room, where the thermometer stood at 62°. In 40 minutes the ice was thawed, and in 95 minutes the acid was liquefied, at the end of which time the thermometer, which stood near the glasses, had risen to 64°. It appears then, that the congealed acid requires more than twice the time for its liquefaction, when exposed to that temperature, that ice does; but I do not think that we can infer, that the heats absorbed and rendered *latent*, as some late philosophers express them-

ſelves; or, in other words, that the cold generated by the liquefaction of ice and of congealed acid are in the above proportion of the times, from the following conſideration; that, as during the liquefaction of the ice, its temperature remains ſtationary at 32°, and during the liquefaction of the acid, its temperature remains about 44° or 45°, the ice, being conſiderably colder than the acid, will take the heat from the contiguous air much faſter.

The experiment does however ſhew, that a conſiderable quantity of cold is generated by the liquefaction of this acid; and hence it appears probable, that in making experiments of producing cold artificially, by mixing ſnow with acids in very cold temperatures, it would probably be uſeful to employ a vitriolic acid of the proper denſity for congelation, and to freeze it previouſly to its mixture with ſnow.

It muſt not, however, be imagined, that the cold generated by the mixture of theſe two frozen ſubſtances is nearly equal to the ſums of the colds generated by the ſeparate liquefactions of the congealed acid and ice, when ſingly expoſed to a thawing temperature: for the mixture reſulting from the liquefaction, conſiſting of the vitriolic acid and the water of the ſnow, appears, from the generation of heat which occurs in the mixture of theſe ingredients in a fluid ſtate, to be ſubject to different laws relatively to heat, than either of the ingredients ſeparately. And the heat, thus generated, as ſoon as the congealed acid and ice are brought to a fluid ſtate, muſt counteract, in ſome meaſure, the cold produced by the liquefaction.

The vitriolic acid, like water and other fluids, is capable of retaining its fluidity when cooled conſiderably below its freezing point. I placed a phial, containing ſome congelable vitriolic

vitriolic acid, in a mixture of ſalt, ſnow, and water; and ſoon afterwards, while the acid was yet fluid, I immerſed in it a thermometer, the mercury of which quickly ſunk from 50° to 29°. While I was moving the thermometer in the fluid, in order to make it acquire the exact temperature, I ſaw the mercury ſuddenly riſe, and upon looking at the acid, I obſerved numberleſs ſmall cryſtals floating in it, which had been ſuddenly formed. The degree to which the mercury then roſe was 46°½. Another time, while the acid was freezing, the thermometer placed in it ſtood at 45°.

From the above obſervations, the following inferences may be drawn.

1ſt, That the vitriolic acid has *a point of eaſieſt freezing*; that is, there is a certain ſtrength or denſity, at which this acid freezes with conſiderably leſs cold than at any other ſtrength, greater or leſs; and that this denſity is nearly to that of water as 1780 is to 1000.

2dly, That the greater or leſs diſpoſition of congelation of the vitriolic acid, which is free from the ſmoking quality that is peculiar to the acid obtained by diſtilling martial vitriol, does not depend on any other quality or circumſtance than its ſtrength or denſity.

3dly, That the freezing and thawing degree of the moſt congelable acid is about 45° of FAHRENHEIT's ſcale. It is, however, to be obſerved, that this degree is inferred from the temperature indicated by the thermometers immerſed in the freezing and thawing acids; but that I never effected the congelation of the fluid acid, without expoſing it to a greater cold, namely, either that of melting ſnow, or of the external air in froſty weather.

Like water, this acid poſſeſſes the property of retaining its fluidity when cooled ſeveral degrees below its freezing point; and of riſing ſuddenly to this point, when its congelation is promoted by agitation, or by contact with even a warmer thermometer.

4thly, That, like water and other congelable fluids, the vitriolic acid generates cold during its liquefaction, and heat during its congelation; the quantity of which heat and cold, ſo generated, remains to be determined by future experiments.

5thly, That the acid, by congelation, when the circumſtances for diſtinct cryſtallization are favourable, aſſumes a regular cryſtalline form, a conſiderable ſolidity and hardneſs, and a denſity much greater than it poſſeſſed in a fluid ſtate.

With reſpect to the firſt mentioned ſpecies of congelation, which is peculiar to the ſmoking vitriolic acid that is procured from martial vitriol, although I have had no opportunity of ſeeing it, as all the vitriolic acid, that is uſed in this country, is obtained by burning ſulphur, yet I will beg leave to ſuggeſt, that it may be worth the attention of thoſe chemiſts to whom it occurs, to obſerve more accurately than has been done, the freezing temperature and the denſity of the congelable acids; and to examine whether the denſity of this ſmoking acid alſo is connected with the glacial property. It ſeems further to be deſerving of inveſtigation, whether there is not ſome analogy between the congelation of the ſmoking oil of vitriol, and the very curious cryſtallization which Dr. PRIESTLEY obſerved in a concentrated vitriolic acid, ſaturated with nitrous acid vapours *; and whether this ſmoking quality does

* Experiments and Obſervations relating to various Branches of Natural Philoſophy.

does not proceed from ſome marine or other volatile acid, which may be contained in the martial vitriol, whence the vitriolic acid is obtained.

Philoſophy, vol. I. p. 26. and 450. M. Cornette has alſo effected the cryſtallization of vitriolic acid, by diſtilling it with nitrous acid and charcoal. Memoir. de l'Acad. des Scienc. Paris, pour 1779.

XXVI. *An Account of ſome new Experiments on the Production of artifical Cold. In a Letter from* Thomas Beddoes, *M. D. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 10, 1787.

DEAR SIR, Oxford, May 2, 1787.

MR. WALKER, Apothecary to the Radcliffe Infirmary here, has been engaged upwards of a year in a ſeries of experiments on the means of producing artificial cold, ſeveral of which ſeem to me to be very remarkable, and ſuch as, conſidering their novelty, and the attention which has lately been paid to this ſubject, I flatter myſelf, will be found to deſerve a place among the Tranſactions of the Society over which you preſide.

Mr. WALKER, in his firſt experiments, found, as BOERHAAVE had done before him, that ſal ammoniac, as well as nitre, well dried in a crucible, and reduced to a fine powder, will produce a greater degree of cold than if they had not received this treatment. But BOERHAAVE, by ſal ammoniac, lowered the temperature of water only by 28°; whereas Mr. WALKER obſerved his thermometer to fall 32°, and when he uſed nitre 19°. It occurred to him, that the combination of theſe ſubſtances would produce a greater effect than either ſeparately: and he found that this was really the caſe. A propoſal for freezing water in ſummer, mentioned by Dr. WATSON (Eſſays, III. 139.) determined him to attempt the ſame thing

thing in this way. Accordingly, April 28, 1786, the thermometer ſtanding at 47°, he made a ſolution of a powder, conſiſting of equal parts of ſal ammoniac and nitre, in a baſon, by means of which he cooled ſome water, contained in a glaſs tumbler, to 22°. To this he added ſome of the ſame powder, and immerſed two very ſmall phials in it; one containing boiled, the other unboiled water; when he ſoon found the water in the phials to be frozen, the unboiled freezing firſt.

Having obſerved that GLAUBER's ſalt, when it retains its water of cryſtallization, produces cold during its ſolution, he thought of adding this to his other powers, and July 18, 1786, reduced the thermometer 46 degrees. In this experiment the following proportions were uſed: the temperature of the air being 65°, to water four ounces, at 63°, were added,

Of ſal ammoniac ℥ xi	thermometer ſunk to 32°,	that is,	31°
Of nitre ℥ x	- - -	24°, that is,	8°
Of GLAUBER's ſalts ℥ ij	- -	17°, that is,	7°
			46°

In this way he froze water on a day ſo hot that the thermometer in the ſhade ſtood at 70°. By firſt cooling the ſalts and water in one mixture, and then making another of theſe cooled materials, he ſunk the thermometer 64 degrees.

Auguſt 28. The temperature of the air being 65°, half an ounce of rectified ſpirit of wine was diluted with three ounces and an half of water, and immerſed in the ſame frigorific mixture. When cooled to 24°, it began to freeze. A quantity of the neutral ſalts, likewiſe cooled in the mixture, were put into the diluted ſpirit, when the thermometer fell to −4°, ſo that the liquor was cooled 69 degrees.

Spirit of nitre, diluted in the manner deſcribed by Mr. CAVENDISH (Phil. Tranſ. vol. LXXVI. part I.), having reduced the thermometer to $-3°$, ſal ammoniac was added, upon which it fell to $-15°$.

Nitrated volatile alkali, during its ſolution in water, reduced the thermometer 35 degrees (from 50° to 15°); but the cold was not increaſed by ſal ammoniac or nitre.

Mr. WALKER's moſt remarkable experiment was made on the 21ſt of March, 1787, when he found that nitrous acid, when poured upon GLAUBER's ſalt, produced effects nearly the ſame as when it is poured on pounded ice; and that the cold, thus produced, is rendered ſtill more intenſe by the addition of ſal ammoniac in powder.

Mr. WALKER, by many trials, diſcovered that the beſt proportion of theſe ingredients is the following: of concentrated nitrous acid, 2 parts by weight, of water 1 part; of this mixture cooled to the temperature of the atmoſphere eighteen ounces, of GLAUBER's ſalt a pound and an half (avoirdupois), and of ſal ammoniac twelve ounces. On adding the GLAUBER's ſalt to the nitrous acid, thus diluted, the thermometer fell from $+51°$ to $-1°$, or 52 degrees; and on adding the ſal ammoniac it fell to $-9°$, that is full 60 degrees. Nitrated volatile alkali, employed inſtead of ſal ammoniac, produced a cold rather more intenſe.

By means of this mixture, in a very few minutes, in the elaboratory before the claſs, I froze ſome ſpirits above proof, diluted with an equal bulk of water; and another gentleman this day ſunk the thermometer 68 degrees.

On April 20, 1787, Mr. WALKER effected the congelation of quickſilver by a combination of theſe mixtures, without a particle of ſnow or ice. When he began his experiment the temperature

temperature of the mercury was 45°, so that, the freezing point of that metal being − 39°, there were produced 84 degrees of cold.

This experiment was performed as follows. Four pans, of sizes progressively diminishing, so that one might be placed within the other, were procured. The largest of these pans was placed in another vessel still larger, in which the materials for the second frigorific mixture were thinly spread, in order to be cooled. The second pan, containing the liquor (*viz.* vitriolic acid, properly diluted) was placed in the largest pan. The third pan, containing the salts for the third mixture, was immersed in the liquor of the second pan; and the liquor for the third mixture was put into wide-mouthed phials, which were immersed in the second pan likewise, and floated round the third pan. The fourth pan, which was the smallest of all, containing its cooling materials, was placed in the midst of the salts of the third pan.

Of the materials for the mixtures to be made in these four pans, the first and second consisted of diluted vitriolic acid and GLAUBER'S salt, the third and fourth of diluted nitrous acid, GLAUBER'S salt and sal ammoniac, in the proportions assigned.

The pans being adjusted in the manner above described, the materials of the first and largest pan were mixed: this mixture reduced the thermometer to + 10, and cooled the liquor in the second pan to + 20; and the salts for the second mixture, which were placed underneath in the large vessel, nearly as much. The second mixture was then made with the materials thus cooled, and it reduced the thermometer to 3°. The ingredients of the third mixture, by immersion in this, were cooled to + 10°, and when mixed reduced the thermometer to − 15°. The materials for the fourth mixture were cooled by immersion in this third mixture to about − 12°. On mixing they made the mercury in the thermometer sink rapidly, and as it

appeared to Mr. WALKER, below −40°. Its thread ſeemed to be divided below that point; but the froth occaſioned by the ebullition of the materials prevented his making ſo accurate an obſervation as he could have wiſhed.

The reaſon why this laſt mixture reduced the thermometer more than the third, though both were of the ſame materials, and the laſt at a lower temperature, Mr. WALKER imagines to have been partly becauſe the fourth pan had not another immerſed in it to give it heat, and partly becauſe the materials were reduced to a finer powder.

I ſhould imagine, that mercury reduced to its freezing point will freeze more quickly than water reduced to its freezing point, becauſe it appears, from experiments on their capacity for heat, that the latter of theſe bodies has ſo much more latent heat in its liquid ſtate; which greater quantity of latent heat muſt, as it becomes ſenſible, more retard the congelation.

I forbear to enumerate many variations of theſe experiments which Mr. WALKER has among his notes; but there is one mixture which, though its power is not equal to that which I have laſt deſcribed, may prove very ſerviceable in experiments of this nature, on account of its cheapneſs. It conſiſts of oil of vitriol diluted with an equal weight of water: added to GLAUBER's ſalt, it produces about 46 degrees of cold. The addition of ſal ammoniac renders it more intenſe by a few degrees. One remarkable circumſtance occurred to Mr. WALKER, as he was endeavouring to aſcertain the beſt ſtrength of the vitriolic acid: he happened to be trying a mixture of two parts of oil of vitriol and one of water, when he obſerved, that, at the temperature of 35°, the mixture coagulated as if frozen, and the thermometer became ſtationary; but, on adding more GLAUBER's ſalt, it fell again, after ſome little time, but ſo great a

cold was not produced as when this circumſtance did not occur, and when the acid was weaker. The ſame appearance of congelation took place with other proportions of acid and water, at other temperatures.

Mineral alkali, when it retained its water of cryſtallization, added to ſome of theſe mixtures heightened their effects. But when it had loſt this water, it rather produced heat than cold; and the ſame thing is alſo true of GLAUBER's ſalt. This circumſtance leads us, in ſome meaſure, to the theory of theſe phænomena. Water undoubtedly exiſts in a ſolid ſtate in cryſtals; it muſt therefore, as in other caſes, abſorb a determinate quantity of fire, before it can return to its liquid ſtate. On this depends the difference between GLAUBER's ſalt and foſſil alkali in their different ſtates of cryſtallization and efflorescence. The ſame circumſtance too enables us to underſtand the great effect of GLAUBER's ſalt, which, as far as I recollect, has the greateſt quantity of water of cryſtallization.

Thoſe, therefore, who ſhall chooſe to purſue the path which Mr. WALKER has opened to them, would do well to try combinations of ſalts containing much water of cryſtallization; but they muſt take care leſt the effect ſhould be diminiſhed or deſtroyed by the formation of compounds that fix a ſmaller quantity of fire. It is, however, but juſtice to Mr. WALKER to obſerve, that he has carried his experiments in this way very far, and with great ingenuity.

I have the honour to be, &c.

THOMAS BEDDOES.

XXVII. *An Account of a Doubler of Electricity, or a Machine by which the leaſt conceivable Quantity of poſitive or negative Electricity may be continually doubled, till it becomes perceptible by common Electrometers, or viſible in Sparks. By the Rev.* Abraham Bennet, *M. A.; communicated by the Rev.* Richard Kaye, *LL.D. F. R. S.*

Read May 10, 1787.

THE great importance of a machine for the purpoſe of detecting very minute quantities of electricity has occurred to many of the cultivators of this ſcience; as by ſuch an aſſiſtant not only many chemical combinations or ſolutions, but alſo many yet unexplained atmoſpherical phænomena, may become intelligible.

The labours of M. Volta have been very ſucceſsful on this ſubject by the application of his condenſer (as he terms it), which, by means of a thin-coated electric, is capable of receiving a greater quantity of the electrical fluid than a common inſulated conductor, and rendering it perceptible by ſeparating the poſitive and negative ſides of the charged plate. On this ingenious contrivance Mr. Cavallo made a very conſiderable improvement by transferring the received quantity of electricity from a larger to a ſmaller condenſer, as explained in the Phil. Tranſ. Vol. LXXII.

Notwithſtanding the very great ſenſibility of this apparatus, the electricity of the atmoſphere is ſometimes too weak to be diſcoverable by it: for inſtance, in ſome ſhowers, when the negative ſtate of the falling rain is nearly equal to the poſitive ſtate of the air. Add to this the trouble of keeping an inſulated and elevated conductor ſufficiently dry, and the danger

ger of it in a thunder-ſtorm. I therefore contrived the following doubler for the purpoſe of more eaſily making an electrico-meteorological diary, which I undertook at the requeſt of my friend Dr. DARWIN, who hoped, that from thence ſome lights might be thrown on the cauſes of the ſudden changes of aërial currents, a circumſtance of ſo much importance to the early growth and maturity of vegetation.

I place upon my electrometer, deſcribed in a former Part of the Philoſophical Tranſactions, a circular braſs plate, three or four inches in diameter, poliſhed and thinly varniſhed on the upper ſurface. On this I place another braſs plate, of equal diameter, poliſhed and varniſhed on both ſides, with an inſulating handle attached to one edge of it. A third plate is alſo provided, of equal diameter, poliſhed and varniſhed on the under ſide, and with a perpendicular inſulating handle from the center of the upper ſide, ſimilar to thoſe mentioned in the Appendix to my laſt Paper.

The method of collecting electricity from the atmoſphere, and continually doubling it as much as required, is as follows. If the weather be dry, I carry into the open air a lighted torch, not liable to be eaſily blown out, or a ſmall lantern with a lighted candle in it, to the bottom of which is fixed, by means of a ſocket, an inſulating handle of glaſs covered with ſealing-wax; in the other hand is carried a coated phial: then, elevating the flame a little higher than my head, I apply to it the knob of the phial, holding it in this ſituation about half a minute. Then returning into the houſe (where the above deſcribed doubler is kept dry, by being placed on a table not far from the fire), I apply the knob of the phial to the under ſide of the firſt plate, which lies immediately upon the electrometer, and at the ſame time touching the ſecond plate with a finger of the other hand. Then laying aſide the phial, I lift up the

the ſecond plate by its inſulating handle, and if the electricity be not now ſenſible by the electrometer, I place the third plate, by means of its inſulating handle, upon the ſecond plate, thus elevated: then touching the third plate, by ſtretching a finger over the juncture of its inſulating handle, and again withdrawing the finger, I then again ſeparate the third plate from the ſecond. In this ſituation it will be apparent to electricians that two of the plates are of one kind of electricity, and nearly of equal quantity, and one only of the other. I then apply the third plate to touch the under ſurface of the firſt plate which remains on the electrometer, and at the ſame time covering the firſt plate with the ſecond, I then touch the ſecond plate by ſtretching a finger over the juncture of its inſulating handle; and firſt taking away the third plate, and then withdrawing my finger from the ſecond, and lifting it up from the firſt plate, the electricity becomes doubled. If by this firſt operation the quantity of electricity does not become ſenſible by the electrometer, I repeat the proceſs to ten or twenty times, which, by doubling it every time, makes viſible the ſmalleſt conceivable quantity of electricity, ſince, at the twentieth operation, it is augmented to above 500,000 times. And though in deſcription the above proceſs of doubling to twenty times may appear tedious, yet when the operator can perform it with ſufficient readineſs (which is ſoon acquired) it takes leſs time than 40 ſeconds. The collection of electricity from the air, and the touching and poſition of the plates, are repreſented in Tab. XI. figures 1. 2. 3. 4. 5. and 6.

If it be required to produce ſparks, the plates are to be placed upon an inſulating ſtand, without an electrometer, and the proceſs repeated as above till the ſparks appear.

The experiment which proves that the electricity is doubled by each operation is this. If the two ſlips of pendulous leaf gold

gold of the electrometer be made to diverge to a certain distance by the above process, that distance will be nearly doubled by repeating the operation. Another proof of this duplicate accumulation is, that, when the third plate is applied to the first, the divergency of the leaf gold is apparently undiminished, though in this situation their electricity is diffused over double the quantity of surface.

That flame will collect electricity better than points was mentioned in my former Paper, and is very evident if two phials of equal capacity are exposed to the air, the one furnished with a sharp point, and the other having its knob applied to an insulated flame, and their electricity afterwards examined by the doubler.

If the weather be rainy, an insulated umbrella may be carried in one hand, and the knob of the phial applied to the upper and insulated part of the handle; and if it rains so slowly as not sufficiently to communicate electricity to the umbrella, a torch is carried under the umbrella, and used as described above.

It is obvious that some caution is necessary in managing experiments of so much nicety, since, by the least friction of the hand on the varnished sides of the plates or insulating handles, or if the metallic side of one plate be accidentally rubbed against the varnished side of the other, some degree of electricity is produced, which, becoming sensible by the operation of doubling, may render the experiments equivocal.

To obviate these inconveniencies, I join a conducting handle, by means of an insulating nut, to each of the plates. This handle consists of turned unbaked mahogany, about three inches long, into one end of which is inserted a nut of baked wood, about half an inch long, covered with sealing-wax, upon the other end of which nut the brass socket of the plate is fixed; by this means it is not necessary to touch the sealing-

wax

wax of the inſulating nut, but occaſionally to ſtretch a finger over it to touch the plate, whilſt the mahogany handle is held in the ſame hand.

Having found, by repeated experiments, that two clean metallic plates, or two equally varniſhed plates, rubbed together, produced no electricity, I varniſhed the ſecond plate on both ſides, but more thinly than when one ſide only was varniſhed, and in ſome experiments uſed thimbles on the ends of the touching fingers. In this way the inconveniencies of accidental friction were in ſome meaſure obviated, but much leſs than I firſt expected: for, notwithſtanding the utmoſt care, electricity is produced without previous communication: therefore, in experiments requiring the electricity to be often doubled, its communication may yet be aſcertained by applying it to the firſt and ſecond plates alternately; ſo that poſitive electricity communicated to the firſt plate appears poſitive by the electrometer; but the ſame electricity, applied to the ſecond plate whilſt the firſt is touched, produces negative in the electrometer.

I beg leave to add, that this method of doubling either poſitive or negative electricity, as well as M. VOLTA's condenſer, with Mr. CAVALLO's improvement on it, as alſo the ingenious experiments of Father BECCARIA with double plates of glaſs, which he ſeparated after charging, are all of them to be explained from the ſame principles with the Leyden bottle, of which they may be all ſaid to be only different applications. I ſhall not therefore trouble the Society with any further theory on this ſubject, but proceed to lay before them the diary which I have hitherto kept, and during which time I have found no difficulty in collecting electricity from the atmoſphere poſitive or negative, ſo as to become ſufficiently ſenſible by the above deſcribed apparatus, though the hygrometer has ſometimes ſhown the greateſt degree of moiſture.

Diary

Diary of atmospherical

Time of observation.	Weather.	Winds.	Barometer.	Thermometer.	Hygrometer.	Electricity.	Instruments to collect.
1787.			in.	°	In.		
Jan. 23. 11 o'clock morning	Cloudy	W. moderate	29.3	44	0⅛ M	Positive	Torch and bott
24. ½ past 10 morning	Thin clouds	N. moderate	29.44	42	0⅛ M	Positive	D°
25. 11 morning	Thin clouds	E. gentle	29.51	37	0⅛ D	Positive	D°
25. 12 at night	Hard froſt and aurora borealis	E. gentle	29.50	38	0⅛ D	Positive	D°
26. 11 morning	Clear froſt	N. gentle	29.5	38	0⅛ D	Positive	D°
26. ½ past 8 at night	Beginning to ſnow	N.E. briſk	29.5	37	0⅛ D	Positive	Umbrella
27. 11 morning	Cloudy and likely to ſnow	N.E. gentle	29.2	35	0⅛ D	Positive	Torch and bott
28. 10 morning	Snowing	S.E. ſtrong	28.8	28	0⅛ D	Negative	Umbrella and to
28. 40 min. paſt 12 noon	Snowing very little	S.E. briſk	28.5	28	0⅛ D	Positive	Torch and bott
28. 11 at night.	Fair, but over-caſt	S. gentle	29.06	37	0	Positive	D°
29. 10 morning	Small rain and miſt on the hills	S. briſk	29.06	38	3⅛ M	Negative	D°
29. ½ paſt 10 morning	Rain ceaſed	D°	29.06	38	3⅛ M	Positive	D°
29. 3 afternoon.	Few drops of rain	S. ſtrong	29.06	41	5⅛ M	Positive	D°
30. 45 min. paſt 10 morning	Fair, but over-caſt	S. ſtrong	29.45	44	6⅛ M	Positive	D°
30. 11 morning	D°	D°	29.44	46	7⅛ M	Positive	D°
31. 10 morning	Fair and clear.	S.W. gentle	29.55	44	3⅛ M	Positive	D°
Feb. 1. 9 morning	Thick miſt with fine drops of rain	Calm	29.53	46	2⅛ M	Positive	Umbrella and to
1. 11 morning	Thick miſt	S.W. gentle	D°	D°	D°	Positive	Kite
2. 11 morning	Thick miſt	S.W. gentle	29.4	44	1⅛ M	Positive	Torch and bott
3. 10 morning	Heavy clouds	S.W. briſk	29.2	46	3⅛ M	Positive	D°
3. ½ paſt 11 morning	Small rain	S.W. ſtrong	D°	D°	D°	Negative	Umbrella
4. 8 morning	Clear and froſty	Calm	29.3	42	0⅛ D	Positive	Torch and bott
5. ½ paſt 10 morning	Thick miſt on the hills	S.E. gentle	29.26	38	0⅛ M	Positive	D°
6. 10 morning	Thick miſt and few drops of rain	S. gentle	29.12	40	1 M	Positive	D°
7. ½ paſt 10 morning	Cloudy	S.W. briſk	28.7	46	5⅛ M	Positive	D°
7. ½ paſt 2 afternoon	Small rain	S.W. briſk	28.69	47	4⅛ M	Negative	Umbrella
8. ½ paſt nine morning	Clear and froſty	Calm	29.28	40	0⅛ M	Positive	Torch and bott
9. 12 noon	Cloudy after rain	S. ſtrong	28.75	46	4 M	Positive	D°
9. ½ paſt 1 afternoon	Rain	S. ſtrong	28.68	46	4⅛ M	Negative	Umbrella
10. 9 morning	Rain	S. gentle	28.64	44	1⅛ M	Negative	Umbrella
11. 12 noon	Beginning to rain	S. briſk	28.34	42	2 M	Negative	Torch and bott
12. 12 noon	Fair, with heavy clouds	S. ſtrong	27.8	44	2⅛ M	Positive	D°
13. ½ paſt 9 morning	Very ſmall rain, thin clouds	N.W. briſk	28.1	44	1⅛ M	Negative	D°
14. and 15. abſent.							
16. 10 morn.	Small rain	S.W. gentle	28.83	47	3⅛ M	Negative	D°
17. 10 morning.	Very few thin clouds	W. gentle	29.42	44	0	Positive	D°
18. 1 afternoon	Few white clouds	W. briſk	29.42	48	1 M	Positive	D°
19. 11 morning	Few diſtant clouds	N.W. gentle	29.53	47	1⅛ M	Positive	D°
20. 11 morning	Over-caſt	N. gentle	29.47	46	1⅛ M	Positive	D°
21. 3 afternoon	Heavy clouds	N. gentle	29.42	47	1⅛ M	Positive	D°
22. 11 morning	Few white clouds	N.E. gentle	29.4	40	0⅛ D	Positive	D°
23. ½ paſt 10 morning	Over-caſt	E. briſk	29.38	40	1⅛ D	Positive	D°
24. 10 morning	Clear and froſty	S.E. gentle	29.32	42	0⅛ D	Positive	D°
25. ½ paſt 12 noon	Few clouds	N.W. ſtrong	29.23	46	1 M	Positive	Lantern
26. ½ paſt 10 morning	Clear	S. gentle	29.3	45	0⅛ D	Positive	Lantern
27. mentioned afterwards							
28. ½ paſt 12 afternoon	Few white clouds	W. ſtrong	29.07	48	0	Positive	Lantern and bo
Mar. 1. 2 afternoon	Few drops of rain	S.W. ſtrong	28.68	50	3⅛ M	Negative	Umbrella
2. 10 morning	Small rain.	S.W. very ſtrong	28.35	47	4⅛ M	Negative	Umbrella and to

N. B. The thermometer and hygrometer were placed where they were not likely to be altered by accidental h
fixed up three years and upwards, during which time its index has moved within the ſpace o

ents to lect.	Number of times doubled, and other observations.
nd bottle	Plate once lifted up.
°	Ditto.
°	Ditto.
°	Ditto, no ſtronger than before.
°	Ditto; a ſtrong bottle was this day tried, which appeared to collect as well as any.
rella	Doubled.
nd bottle	Plate once lifted up.
and torch	Ditto.
nd bottle	Doubled.
°	Plate once lifted up; divergency of the leaf gold very great.
°	Ditto, but weaker.
°	Four times doubled.
°	Plate once lifted up.
°	Ditto.
°	Ditto; the hygrometer was higher than known during above three years.
°	Ditto.
and torch	Three times doubled.
ite	Strong ſparks were produced from a ſtring 260 yards long, with braſs wire in it.
nd bottle	Plate once lifted up, very ſtrong.
°	Ditto, alſo very ſtrong.
rella	Twice doubled; a few minutes after the torch was tried, when the rain had ceaſed, and a much ſtronger poſitive was produced.
nd bottle	Plate once lifted up.
°	Ditto, ſtrong.
°	So ſtrong as twice to ſtrike the ſides of the electrometer, and ſenſible without the plates.
°	Plate once lifted up.
brella	Ditto.
nd bottle	Ditto.
°	Ditto.
brella	Ditto.
brella	Ditto.
nd bottle	Ditto.
°	Ditto.
°	Ditto; 10 minutes after the rain ceaſed the electrometer was weakly negative, and 5 minutes after that ſtrongly poſitive.
°	Ditto.
°	Ditto.
°	Ditto.
°	Ditto.
°	Ditto.
°	Ditto.
°	Ditto.
°	Ditto.
°	Ditto.
ntern	Juſt ſenſible to the electrometer, without the doubler.
ntern	More ſenſible, the lantern being elevated fifteen feet high.
and bottle	Four times doubled.
brella	Often doubled; a few minutes after rain ceaſed, and the electrometer became poſitive.
a and torch	Plate once lifted up.

ental heat. The hygrometer conſiſted of fifteen feet of whipcord, ſuſpended horizontally: it has been ſpace of 14 inches, which is therefore divided into inches and eighths, moiſt and dry.

The atmoſpherical electricity was ſometimes ſo ſtrong as to need no doubling, and moſtly required only one application of the ſecond plate, yet I frequently found it neceſſary to repeat the proceſs from two or three to twenty times. Perhaps the exact comparative quantity of electricity reſiding in the atmoſphere might be meaſured by the number of operations required to render it perceptible by the electrometer, all other circumſtances being cautiouſly attended to.

If the electricity of the atmoſphere ſhould happen to be much weaker than I have yet found it, there remains not only the reſource of doubling oftener, but the capacity of the inſtruments may be much increaſed; as, firſt, by uſing a larger flame; ſecondly, by elevating it higher; thirdly, by collecting the electricity with a very thin glaſs ball, ſilvered within, and coated on the outſide in the common way, or gilt; fourthly, by grinding and poliſhing the plates of the doubler very exactly; fifthly, by making the experiments in an advantageous ſituation. In all theſe particulars my apparatus was defective, yet amply ſufficient for the diſcovery of the atmoſpherical electricity.

After conſidering the ſucceſsful effect of flame, in collecting atmoſpherical electricity, I placed an inſulated lantern upon a pole about fifteen feet high, and ſuſpending a gold thread from the lantern connected it with the electrometer, and was agreeably amuſed with ſeeing the pendulous gold leaf open and ſhut with every paſſing cloud.

On the 27th of February, 1787, when there was a conſiderable miſt whilſt the lantern was thus elevated, the leaf gold frequently ſtruck the ſides of the electrometer; and, in

about an hour, ſome drops of rain beginning to fall, the appearance of electricity with this apparatus entirely ceaſed, though I elevated the lantern ſeveral times in the courſe of the day.

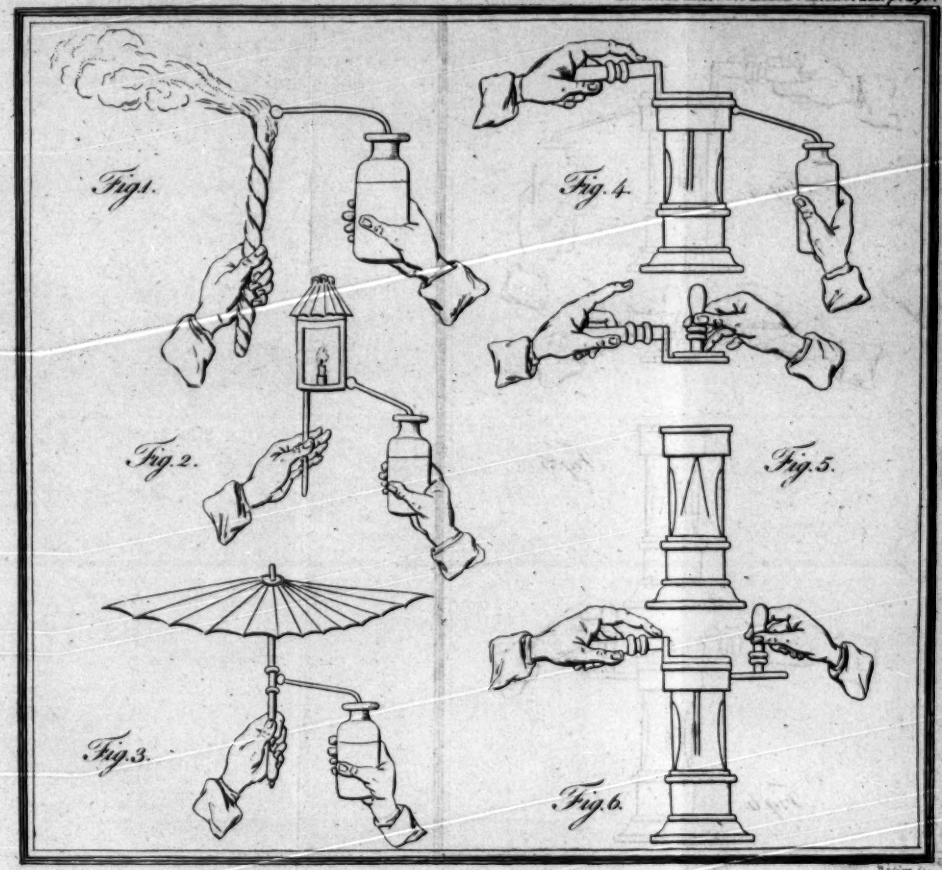


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.

XXVIII. *Some Particulars relative to the Production of* Borax. *In a Letter from* William Blane, *Esq. to* Gilbert Blane, *M.D. F. R. S.*

Read May 17, 1787.

MY journey to the northern mountains in January last, in attendance upon the Vizier, gave me an opportunity of satisfying, in some degree, my curiosity on the subject you are so desirous of being informed of, the production and manufacture of Borax. The place which his Excellency visited is called Betowle, and is a small principality in the first of the northern mountains, where they rise from the plains of Hindostan, and is distant from Lucknow about 200 miles N.E. The town is a principal mart, where the commodities of the mountains are exchanged for those of the plain. The Raja, or Prince of the country, holds his possessions in the hills as an independent sovereign; but for those on the plain he owes fealty, and pays tribute to the Vizier. He therefore embraced this opportunity of paying homage in person to his Lord. During his stay at court, I had an opportunity of making the enquiries I wished from his people, and particularly from his Dewan or Minister, who had with him some of the inhabitants of the place where the borax is made.

This saline substance, called in the language of this country *Swagah*, is brought into Hindostan from the mountains of Tibbet. The place where it is produced is in the kingdom of

Jumlate, distant from Betowle about thirty days journey north. Jumlate is the largest of the kingdoms in that part of the Tibbet mountains, and is considered as holding a superiority over all the rest.

The place where the borax is produced is described to be in a small valley, surrounded with snowy mountains, in which is a lake, about six miles in circumference, the water of which is constantly hot, so much so that the hand cannot be held in it for any time. The ground round the banks of the lake is perfectly barren, not producing even a blade of grass; and the earth is full of a saline matter in such plenty that, after falls of rain or snow, it concretes in white flakes upon the surface, like the *natron* in Hindostan. Upon the banks of this lake, in the winter season, when the falls of snow begin, the earth is formed into small reservoirs, by raising it into banks about six inches high; when these are filled with snow, the hot water from the lake is thrown upon it, which, together with the water from the melted snow, remains in the reservoir, to be partly absorbed by the earth, and partly evaporated by the sun; after which, there remains at the bottom a cake, of sometimes half an inch thick, of crude borax, which is taken up and reserved for use. It can only be made in the winter season, because the falls of snow are indispensably requisite, and also because the saline appearances upon the earth are strongest at that season. When once it has been made upon any spot, in the manner above described, it cannot be made again upon the same place, till the snow shall have fallen upon it and dissolved three or four times; after which the saline efflorescence re-appears, and it is again fit for the operation.

The borax, in the state above described, is transported from hill to hill upon goats, and passes through many different hands before

before it reaches the plains, which increaſes the difficulty of obtaining authentic information regarding the original manufacture. When brought down from the hills, it is refined from the earth and groſs impurities by boiling and cryſtalliſation. I could obtain no anſwers to any queſtions regarding the quality of the water, and the mineral productions of the ſoil. All they could ſay of the former was, that it was very hot, very foul, and as it were greaſy; that it boils up in many places, and has a very offenſive ſmell: and the latter remarkable only for the ſaline appearances above deſcribed. That country, however, in general, produces conſiderable quantities of iron, copper, and ſulphur. After being purified it ſells in the market here for about 15 rupees *per* maund; and I am aſſured, by many of the natives, that all the borax in India comes only from the place above mentioned.

I am afraid you will think this at beſt but a very unſatisfactory and unphiloſophical account of the matter; but what can be done, where the only mode of information is through ſome of the wild and unſettled mountaineers? for the place is inacceſſible even to the inhabitants of Hindoſtan, and has never been viſited by any of them, except a few wandering Faquires, who have been ſometimes led that way, either to do penance, or to viſit ſome of the temples in the mountains. The cold in winter is deſcribed to be ſo intenſe that every thing is frozen up, and that life can only be preſerved by loads of blankets and ſkins. In the ſummer again, the reflection from the ſides of the mountains, which are ſteep and cloſe to each other (there being little or no plain ground betwixt them), renders the heats inſufferable.

I have not loaded this account with any reflections or conjectures of my own. I have ſimply given you the narrative of thoſe from whom I had my information; and having put into your

your poſſeſſion all the *data* I have been able to collect upon the ſubject, you may make what uſe of them you pleaſe.

I ſhall conclude with a few obſervations regarding the credibility of the relation: and, firſt, that it is really brought from the Tibbet mountains is certain, as I have myſelf often had occaſion to ſee large quantities of it brought down, and have purchaſed from the Tartar mountaineers, who brought it to market; ſecondly, I have never heard of its being either produced or brought into this country from any other quarter; and, thirdly, if it was made on the Coromandel coaſt, as ſome books mention, I think there can be little doubt, but that the whole proceſs would have been fully enquired into, and given to the public long before this time.

Lucknow, Auguſt 28, 1786.

XXIX. *A Letter from the Father Prefect of the Miſſion in* Thibet, F. Joſeph da Rovato, *containing ſome Obſervations relative to* Borax. *Communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 17, 1787.

Alli illuſtriſſimi Signori Membri dell' Accademia Reale delle Scienze ed Arti di Londra.

IL Padre prefetto della Miſſione del Tibet ha il piacere di manifeſtare alle SS. VV. illuſtriſſime, come eſſendo in Patna ſtato ricercato più volte dal Sig. Vogles, Alemano, bravo naturaliſta, di prendere notizia dei luoghi, e del come ſi faceva il Borace, che ſi ha dal regno del gran Tibet; giacchè come egli diceva neſſun alto poteva avere comunicazione da quelle parti quaſi impenetrabili; quantunque la noſtra miſſione abbia abbandonato da molti anni detto regno, nonoſtante avendo amicizia con il *Bahadur Sah*, fratello del re di Nepal (il regno del quale ſi eſtende a tramontana fino a Kutti, frontiera del Tibet) li ſcriſſe, e lo pregò a prendere la notizia dei luoghi, e del come ſi faceva il borace, ed a comunicargliela. Il Bahadur Sah avuta queſta iſtanza, per meglio favorire il Padre prefetto, avendo al ſuo ſervizio un uomo dello ſteſſo paeſe dove ſi forma il borace,

si degnò di mandarglielo fino a Patna, per intendere dallo stesso uomo tutto ciò che desiderava intorno al borace.

Questo uomo, parte in lingua Nepalese, parte in lingua Indostana, ambedue intese dal Padre prefetto, si spiegò come segue. Nella provincia, ossia territorio di Marmè, distante 28 giornate di camino a tramontana del Nepal, e 25 dette al ponente di Lassa capitale del Tibet, vi è una valle larga 8 miglia in circa. In un distretto di questa valle vi sono due terre, o Castelli, uno chiamato *Scierugh*, e l'alto *Kanglè*; e li uomini di questi due luoghi s'impiegano a cavare il borace, e lo vendono nel Tibet, e nel Nepal, non avendo altro mezzo per vivere, stante che in detta valle il terreno è sterile, e non produce che alcuni giunchi. Vicino ai sudetti due Castelli vi è una Vasca di acqua, che non è molto grande, e varie altre più piccole (dove il terreno è concavo) nelle quali resta l'acqua quando piove. In queste Vasche dunque, dopo qualche tempo che vi è restata l'acqua, si forma da per se il borace; e li uomini entrando nell' aqua, dove sentono con li piedi come un lastrico, ivi il borace è formato, e lo cavano; con questa distinzione, che dove è poca acqua, il borace è più sottile, e lo sentono subito; e dove l'acqua è molto lo trovano più grosso, ed a questo vi resta sopra un dito, o due, di fango molle, che sarà una deposizione della stessa dopo ch' è stata intorbidata dalla pioggia, o dal vento. Così si trova il borace prodotto dalla natura senza uso alcuno nè di bollitura, nè di lambicco; e l'acqua nella quale si produce il borace è così cattiva, che se alcuno ne beve un poco, li gonfia il ventre e lo fa morire. La terra, in cui si produce il borace, è di colore bianchiccio; e 4 miglia in distanza dove si produce il borace, nella stessa valle vi sono le miniere del sale, che si cava in grande abbondanza per uso di tutti quei popoli dentro le monti, che

sono

ſono coſi lontani dal mare. Li nativi non pagano nulla a cavare il borace, non avendo altro mezzo per vivere a cauſa della ſterilità del terreno; ma ſe li ſtranieri vogliono cavare il borace è neceſſario che paghino un tanto al Capo del luogo ſecondo la convenzione che fanno; e nella valle di Marmè pagano un Lamà chiamato Pema *tupkan*, a cui appartengono le miniere del borace.

Altre 10 giornate di camino più a tramontana della valle di Marmè vi è un altra valle chiamata Taprè, nella quale pure ſi cava il borace. Vi è ancora un altro luogo, in cui ſi cava il borace, chiamato *Ciogà*, ma di queſto ultimo non ho marcata la ſituazione. Il borace in lingua Indoſtana e Nepaleſe ſi chiama *Soagà*; ma ſe non è purificato ſvaniſce facilmente; e per conſervarlo qualche tempo finchè lo vendono, lo miſchiano con la terra unta di buttiro.

Nel territorio pure di *Mungdan* 16 giornate più a tramontana del Nepal vi ſono abbondanti miniere dell' arſenico; ed in molti altri luoghi le miniere del ſolfo, come pure le miniere dell' oro, e dell' argento, che ſi cava aſſai più puro di quello che ſi cava dalle miniere del Pegù.

Queſto è quanto ſi è ricavato dal uomo mandato dal Bahadur Sah, fratello del re di Nepal. E ſe gli Signori dell' Accademia Reale deſiderano di videre un poco di quella terra, nella quale ſi produce il borace, ora che tutto il paeſe di Nepal è governato dallo ſteſſo Bahadur Sah; il Padre prefetto, che lo ha praticato per alcuni anni dentro lo ſteſſo Nepal, e due volte ancora a Patna (ove era venuto) ſpera che non gli negarà il favore di mandare qualche ſuo fidato uomo a prendere di detta terra, e inviargliela a Patna; dal qual luogo potrà il Padre prefetto, mediante qualche Sig. Ingleſe ſuo amico, fargliela con tutta facilità pervenire a Londra.

Queſto è quanto il Padre prefetto prende la libertà di ſcrivere alli illuſtriſſimi Signori dell' Accademia, alli pure ſi offeriſce di

buon cuore con li altri suoi Religiosi Cappuccini Italiani, se potrà servirli per qualche altra notizia di questo genere, per mostrare la sua gratitudine alla nazione Inglese, dalla quale ha ricevuti, e riceve tanti benefizi; ed ha l'onore di raffermarsi con tutto il rispetto, &c.

F. GIUSEPPE DA ROVATO.

Patna, li 10. 7bre, 1786.

XXX. *Sur les Gas Hepatiques: par Monsieur* Hassenfratz. *Communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read May 17, 1787.

EN 1785, après avoir fait quelques expériences sur les foies de soufre, je résolus de chercher quel étoit la nature de l'air inflammable hépatique que l'on en faisait dégager en les décomposant par l'acide nitreux. Le soufre que l'on voit se précipiter après chaque combustion de gas inflammable hépatique me portait à croire que cette substance pouvait bien être une de ses parties constituantes, mais elle ne m'en apprenait pas davantage, et c'étoit dans ce tems à-peu-près la seule expérience que l'analyse pouvait me fournir. Je résolus donc d'employer la synthèse, et ce qui me conduisit à cette résolution fut la citation d'une expérience que M. Monges avait fait à Mezieres; il avait fait passer de l'air fixe à travers du soufre en fusion, et il avait eu pour résultat de l'air fixe avec une odeur sulfureuse, et qui tenait réellement du soufre en dissolution. Je me déterminai donc à répéter son expérience, et à tenter celle que je vais citer.

Je fis d'abord passer de l'air fixe à travers du soufre en fusion, at j'eus pour résultat un air fixe sulfureux qui faisait précipiter la chaux vive de sa dissolution dans l'eau distillée, en s'unissant à cette base, formant par cet union, de la terre calcaire, et laissant dégager le soufre qui surnageait le liquide, ou se précipitait au fond. Je fis la même expérience avec le gas nitreux, et j'eus pour

réſultat un gas nitreux ſulfureux, qui ſe combinait encore avec l'air vital pour former de l'acide nitreux, et abandonnait le ſoufre dans cette combinaiſon.

La moſète atmoſphérique m'a produit, après ſon paſſage, de la moſète ſulfureuſe avec odeur de ſoufre, qu'elle laiſſait précipiter après avoir ſéjourné quelque tems ſur l'eau.

L'air vital a produit dans une ſemblable expérience un air vital ſulfureux, mélangé d'acide ſulfureux volatil, que l'eau abſorbait : le gas nitreux, en s'uniſſant avec rutilance à l'air vital ſulfureux, en faiſait dégager le ſoufre, de même que ſa détonnation avec le gas inflammable.

L'air atmoſphérique m'a donné un réſultat à-peu-près ſemblable à l'air vital, ſeulement, ainſi qu'il eſt aiſé de le déduire des expériences précédentes, l'air que j'obtins était un mélange d'air vital et de moſète ſulfureuſe. Le gas nitreux par ſon union avec l'air vital et le gas inflammable dans ſa détonnation faiſait précipiter le ſoufre de ſon mélange.

Enfin le gas inflammable, paſſé à travers du ſoufre en fuſion, m'a donné un gas inflammable ſulfureux, tout-à-fait ſemblable au gas hépatique que l'on obtient en verſant de l'acide nitreux ſur du foie de ſoufre : d'où il paroit ſuivre que ce gas inflammable que l'on retire des foies de ſoufre en y verſant de l'acide nitreux, et auquel on a donné le nom de gas hépatique, n'eſt autre choſe qu'un gas inflammable ſulfureux, que l'on peut former ſynthétiquement, ainſi que toutes les autres eſpèces de gas ſulphureux.

XXXI. *Botanical Deſcription of the* Benjamin Tree *of* Sumatra. *By* Jonas Dryander, *M. A. Libr. R. S. and Member of the Royal Academy of Sciences at* Stockholm; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 17, 1787.

THOUGH Garcias ab Horto, Grim, and Sylvius, were acquainted with the real tree from which Benjamin or Benzoin is collected, their deſcriptions of it are ſo imperfect and inſufficient for its botanical determination, that ſucceeding botaniſts have fallen into many errors concerning it; and it is remarkable, that although this drug was always imported from the Eaſt-Indies, moſt of the later writers on the Materia Medica have conceived it to be collected from a ſpecies of Laurus, native of Virginia, to which, from this erroneous ſuppoſition, they have given the trivial name of Benzoin. This miſtake ſeems to have originated with Mr. Ray, who in his Hiſtoria Plantarum, Vol. II. p. 1845. at the end of his account of the Arbor Benivifera of Garcias, ſays: "Ad nos "ſcripſit D. Tancredus Robinson Arborem reſiniferam odo-"ratam foliis citrinis prædictæ haud abſimilem tranſmiſſam "fuiſſe e Virginia a D. Baniſter, ad illuſtriſſimum Præ-"ſulem D. Henr. Compton, in cujus inſtructiſſimo horto "culta eſt.—— Arbor iſta Virginiana Citrii, vel Limonii foliis "Benzoinum fundens, in horto reverendiſſimi Epiſcopi culta."

This error was detected by Linnæus, but another was ſubſtituted by him in its place; for in his Mantiſſa Plantarum

Altera,

Altera, he tells us, that Benjamin is furniſhed by a ſhrub deſcribed there under the name of Croton Benzoe, and afterwards in the Supplementum Plantarum, deſcribes again the ſame plant, under the name of Terminalia Benzoin. M. JACQUIN, who had been informed that this ſhrub was called by the French *Bienjoint*, ſuppoſes, with reaſon, that the ſimilar ſound of that word with *Benjoin*, the French name for Benjamin, may have occaſioned this miſtake *.

Since that period Dr. HOUTTUYN has deſcribed the Benjamin Tree of Sumatra; but for want of good ſpecimens has been ſo unfortunate as to miſtake the genus to which it belongs. It is hoped, therefore, that the following deſcription and annexed figure (ſee Tab. XII.) may not be unworthy a place in the Philoſophical Tranſactions; they are made from dried ſpecimens procured from Sumatra by Mr. MARSDEN, F. R. S. at the requeſt of Sir JOSEPH BANKS, Bart. P. R. S. and clearly prove that this tree agrees in the parts of fructification with the Styrax of LINNÆUS.

STYRAX *Benzoin*, foliis oblongis acuminatis ſubtus tomentoſis, racemis compoſitis longitudine foliorum.

Benjui. *Garcias ab Horto* in *Cluſii Exoticis*, p. 155.

Arbor Benzoini. *Grim* in *Ephemer. Acad. Nat. Curioſ. Dec.* 2. *Ann. I. pag.* 370. *fig.* 31. *Sylvius* in *Valentini Hiſtoria Simplicium*, *pag.* 487.

Benzuin. *Radermacher* in *Act. Societ. Batavicæ*, *vol. III. pag.* 44.

Benjamin or Benzoin. *Marſden's Hiſt. of Sumatra*, *pag.* 123.

Laurus Benzoin. *Houttuyn* in *Act. Harlem. vol. XXI. pag.* 265. *tab.* 7.

Habitat in Sumatra. *h.*

* Hort. Vindob. vol. III. p. 51.

DESCRIPTIO.

Styrax Benzoin.

Bayard &c.

DESCRIPTIO.

Rami teretes, tomentoſi.

Folia alterna, petiolata, oblonga, integerrima, acuminata, venoſa, ſupra glabra, ſubtus tomentoſa, palmaria. *Petioli* teretes, ſtriati, canaliculati, tomentoſi, breviſſimi.

Racemi axillares, compoſiti, longitudine fere foliorum. *Pedunculi communes* tomentoſi; *partiales* alterni, patentes, tomentoſi. *Pedicelli* breviſſimi. *Flores* ſecundi.

Calyx campanulatus, obſoletiſſime quinquedentatus, extus tomentoſus, linea longior.

Petala quinque, (baſi forte connata) linearia, obtuſa, extus tomento tenuiſſimo cinerea, calyce quadruplo longiora.

Filamenta decem, receptaculo inſerta, petalis paulo breviora, inferne connata in cylindrum longitudine calycis, ſuperne infra antheras ciliata. *Antheræ* lineares, filamentis longitudinaliter adnatæ, iiſque dimidio breviores.

Germen ſuperum, ovatum, tomentoſum. *Stylus* filiformis, ſtaminibus longior. *Stigma* ſimplex.

XXXII. *An Account of an Experiment on Heat. By* George Fordyce, *M. D. F. R. S. In a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 24, 1787.

SIR,

HEAT changes the qualities and appearances of matter in various ways. It is alſo a powerful agent in many of the operations which mankind employ to fit matter for their uſe. Although the ancients performed many of theſe operations with a conſiderable degree of accuracy, yet there are many which they were totally unacquainted with, and others they brought to little perfection. One principal cauſe was their having no means of meaſuring heat accurately. VAN HELMONT was the firſt who found the mode of meaſuring heat by expanſion. His meaſure was an air thermometer, which is deſcribed in his Diſſertation, named "Aer", *cap.* 12. Since his time, various improvements have been made on thermometers; many are ſtill wanted. This inſtrument is, however, the foundation of modern diſcoveries on this ſubject. The ancients were acquainted with the manner of heating bodies by communication, by friction, by burning fuel, by the ſun, by fermentation, and the taking place of chemical combinations in other caſes. BOYLE found, that melting a ſolid body produced cold (Experimental Hiſtory of Cold, title I. chap: 18.):

Dr. CULLEN, that cold was alſo produced by converting bodies into vapour. It has been ſince that time found, that the oppoſite condenſations, *viz.* of vapours into fluids, or fluids into ſolids, generate heat. CRAMER was the firſt who took notice of the different conducting powers of different bodies, in his "Ars "Docimaſtica," P. I. § 274. Scholium.

The power of animal bodies, of reſiſting the cold of the medium they are in, has been long known. Mr. ELLIS took notice of their being alſo able to reſiſt the heat. Dr. CULLEN aſcribed this power to a peculiar quality in animals different from the powers of inanimate matter. You, Sir, ſaw a confirmation of this power being very great when we kept a dog of no large ſize (he might weigh, as far as I can recollect, about twenty-five pounds, not more) in air heated to 160 degrees of FAHRENHEIT's thermometer for half an hour. We took him out with only the addition of a few degrees of heat; not from any uneaſineſs of the animal, but from being ſatisfied with the experiment. This power has been ſhewn by Mr. HUNTER to extend to vegetables. The degree of heat one body is capable of impregnating another with, was hardly touched upon by any author before Dr. CRAWFORD, who has done a great deal in this branch, and is ſtill purſuing it.

The ſubject of the preſent enquiry is different from all theſe. The propoſition is, ſuppoſing we can make an application to a cold body, ſo as to produce heat in it, and this application be made with the ſame force to the ſame body, whether by this means an equal quantity of heat will always be produced in an equal quantity of matter? That is, for inſtance, whether an equal quantity of the rays of the ſun being thrown on an equal ſurface of the ſame matter, ſo that they ſhall be equally loſt, bent, or reflected, an equal maſs of matter below ſhall be

equally heated according to its capacity; whether equal vibrations excited ſhall always produce the ſame quantity of heat; whether a chemical attraction taking place between an equal quantity of two ſubſtances ſhall always produce an equal quantity of heat?

The importance of this enquiry is ſufficiently evident, ſince if the ſame quantity of fuel being burnt the ſame quantity of heat be always produced, our whole attention will be to take care that no part of the heat ſhall be loſt; but if burning the fuel under one ſet of circumſtances will actually produce a greater quantity of heat than burning it in other circumſtances; or if burning it, will produce a great heat in one place, which cannot be carried to another place, but will be again annihilated, a very different attention muſt be paid. I was firſt led into this train of thinking by obſerving reverberatory furnaces. Formerly I had no doubt but that it was obvious, that the ſame quantity of fuel burnt would produce the ſame quantity of heat; but having occaſion to try ſome experiments in reverberatory furnaces, where great heat and cleanneſs were required, I tried to heat the furnace with charcoal and coak, or pit-coal charred, that is, burnt till no ſmoak ariſes, but could never produce the heat required, although I could do it eaſily with coal. I inſulated my furnace, ſo that after twenty-four hours ſtrongeſt fire, it did not feel in the leaſt warm on the outſide. I heightened the chimney; but all to no effect: in the fire-place the heat was ſufficient to melt malleable iron, but in the laboratory, in the horizontal part of the chimney, the heat was trifling. Since that time I have made various experiments to aſcertain the propoſition laid down. The following one, which has been varied and repeated with the ſame reſult, may, perhaps, draw the attention of chemiſts to this point.

I formed a cylinder of thin paſteboard, ſix inches diameter and ſixteen inches long. The inſide I lined with rabbit ſkin, laying the fur ſmooth; a thin ring of paſteboard was placed in the middle. One end was cloſed with a bottom of the ſame paſteboard; the other was open. This cylinder was placed in the center of another wider cylinder, alſo of paſteboard, which had likewiſe a bottom of paſteboard. It was ſo placed, as that the outer cylinder was diſtant from the inner one inch and a half, at the bottom and ſides the ſpace between was filled with Eider down, ſuffered to riſe to as great a bulk as it would from its own elaſticity. The two cylinders were even at top, and the ſpace between them ſhut by a cover of paſteboard. In the ſide of the machine, a little below the middle of the inner cylinder, a paſteboard tube was made to paſs through the outer and open into the inner, half an inch wide, for the inſertion of a thermometer. A ſimilar tube was placed a little from the middle, towards the other end of the ſmalleſt cylinder. A circular plate, of paſteboard, ſix inches diameter, and about one-eighth thick, weighing 1 oz. 102 grs. was puſhed down the inner cylinder, until it was ſtopped by the ring. A circle of flint glaſs, ground flat and parallel on both ſides, was fixed over the mouth of the inner cylinder ſo as not to obſtruct any part of it.

A ſimilar apparatus, as exactly as poſſible, was formed, excepting that the circular plate in the middle of the inner cylinder was iron, of the ſame dimenſions with the paſteboard one, and weighing 12 oz. 62 grs. Theſe apparatus's were ſet in a warm expoſure for ſeveral months, to dry.

The circular plates, which were deſtined to receive the direct rays of the ſun, were placed as nearly perpendicular to the inner cylinder as poſſible. They were both covered with a

black paint, ſufficient to prevent the rays of the ſun from penetrating either to the iron or paſteboard.

On the 28th day of July, 1786, the ſun ſhining on a room facing about S.W. the air not cloudy, but not very bright; the air in the room 71°; at a quarter after twelve, thermometers being paſſed through the tubes below the plates of iron and paſteboard, after ſtanding a quarter of an hour, ſhewed the heat 67° in both apparatus's. Both were now expoſed to the ſun, ſo that the rays fell perpendicular on the paint covering the plates, in equal quantity on each as nearly as poſſible. If there was any difference, rather more were thrown on the paſteboard diaphragm. In five minutes the thermometer below the paſteboard diaphragm ſhewed 72 degrees; the thermometer under the iron had hardly riſen half a degree.

Progreſs of the riſing of the thermometers.

Under paſteboard diaphragm.	Under iron diaphragm.
72°	67½
75	70
80	76+
85	83
90	88+
95	94
100	100*
105	107
110	115
After 20 minutes,	
110	121

* At this time thermometers were put through tubes into the chambers of the apparatus, between the glaſſes and diaphragms. The apparatus with the iron diaphragm raiſed this thermometer to 121°; that with the paſteboard to 120°.

The

The apparatus with the paſteboard diaphragm was expoſed ſtill to the ſun; that with the iron was removed, and ſuffered to cool till its thermometer ſhewed 107°; it was then expoſed again to the ſun till it had acquired the heat of 110°, to which degree the apparatus with the paſteboard hardly reached. The windows were now ſhut. The heat of the room had ariſen to 80°. Both the apparatus's were placed on a table; the doors were ſhut, ſo that there was no current of air.

Paſteboard apparatus, after 30 minutes.		Iron apparatus.
96		104
After 75 minutes, or 1 h. 45′ from the beginning.		
83		89
After 2 h. or 3 h. 45′ from removal from the ſun.		
78	room 75	80

A ſimilar reſult aroſe when there were no glaſſes to exclude the external air. Likewiſe when the diaphragms were changed from one apparatus to the other.

If any one wiſhes to repeat theſe experiments, he muſt take care that the ſize of both apparatus's be the ſame; the weight the ſame, excepting the difference of the iron and paſteboard; that they be equally ſtuffed, and perfectly dry: for if there be the leaſt moiſture, the evaporation will not only make a fallacy in the experiment, but it will ſoon obſcure the glaſſes, ſo as to prevent the rays of the ſun from paſſing through them.

The firſt thing to be noted in this experiment is, that the rays of the ſun acted on the ſame black paint only: for it was ſo thick, that the rays could not penetrate to the iron or paſteboard below. The colour was the ſame, and there was the ſame gloſs; if any thing, that on the iron, in the experiment related, was rather more gloſſy, in order that it might not be

favoured,

favoured, as in former experiments the results had been in favour of the iron apparatus acquiring the greatest heat. Every thing, therefore, was the same, except that the iron and pasteboard were of different weights, of different capacities of heat, and of different degrees of readiness to acquire heat, and communicate it.

It is evident, that a greater quantity of heat was actually produced in the apparatus with the iron diaphragm: for although in the first two or three minutes the pasteboard became hotter than the iron, yet as soon as the iron began to be sensibly heated, it became hot faster than the pasteboard, and actually became hotter, and even continued to do so, when the pasteboard no longer could produce more heat than was dissipated from the surface of the apparatus into the air. When they were set in an air equally cold the apparatus with the iron diaphragm was longer in cooling, although they were both of the same degree of heat when set by.

This greater quantity of heat I ascribe to the iron's taking the heat from the black paint faster than the pasteboard, as being a better conductor. Just as if a plate of glass was placed on a plate of steel, and another, perfectly similar, was placed on a plate of clay, and both were placed equally among equal vibrating bodies. In this case it is clear, that much greater vibration would take place if the same means of exciting it were applied to that plate of glass attached to the plate of steel than if they were applied to that attached to the clay. I do not mean to say, that heat is vibration; but merely to illustrate my idea of heat's being only a quality, and not a substance. I am led to this not only by this experiment now related, but by various other considerations, which I shall not now insist upon, as they are not sufficiently finished to be laid before this Society.

I shall

I ſhall only add that, among other things which may be illuſtrated by it, one is, that all the planets may poſſibly be of the ſame heat; ſince, if the matter of which Mercury conſiſts was averſe to the generation of heat in proportion to the greater number of the rays of the ſun it receives more than the Georgium Sidus, they would be both of the ſame heat, notwithſtanding their different diſtances from the ſun.

I have already ſaid, that I was led to an enquiry into the ſubject by the effect it has on chemical operations.

I remain, &c.

G. FORDYCE.

XXXIII. *Account of an Obſervation of the Right Aſcenſion and Declination of Mercury* out *of the Meridian, near his greateſt Elongation*, Sept. 1786, *made by Mr.* John Smeaton, *F. R. S. with an* Equatorial Micrometer, *of his own Invention and Workmanſhip; accompanied with an Inveſtigation of a Method of allowing for* Refraction *in ſuch Kind of Obſervations; communicated to the Rev.* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal, and by him to the Royal Society.*

Read June 7, 1787.

M. DE LA LANDE having announced to ſome of my aſtronomical friends the utility of accurate obſervations of Mercury, at his two elongations the laſt year, in Auguſt and September; I tried to get obſervations of that planet in croſſing the meridian, for ſome days before and after the greateſt elongation in Auguſt; and though the ſtate of the atmoſphere about that time was not very favourable to the purpoſe, yet there was one day that I thought unexceptionable, but could not perceive the leaſt appearance of Mercury; at which I was the rather ſurpriſed, as I had formerly ſeen that planet in the like ſituation, with the ſame inſtrument, with perfect perſpicuity *: and as I did not hear of any one elſe having ſucceeded in

* The inſtrument mentioned is a tranſit, made by myſelf in the year 1768; at which time achromatic object-glaſſes not having been, ſo far as I knew, applied to aſtronomical inſtruments by others, but having found the good effects thereof for

in this observation, I thought it might be very possible for the same disappointment again to happen, with respect to the approaching elongation in September. I judged, therefore, that it might be of some utility to astronomy, if, by *any means*, a good observation of Mercury could be got; and also, that it would be a proper subject whereon to make trial of an instrument for such purposes, the idea of which I had conceived, and begun to construct, above forty years before; but which, from various avocations, I did not perfect to my satisfaction till the year 1770*: since which time it has lain by, in hopes that something might happen, by which a full and effectual trial might be made thereof.

This instrument was originally intended as an improvement of the common wire micrometer, for the purposes of taking differences of right ascension and declination, in a more commodious and effectual manner than could be done in the method then practised of using that instrument †; and at the same

for other purposes, I resolved to use a double achromatic object-glass, made by Mr. DOLLOND; which being of equal aperture with the simple object-glass, then in the transit of the Royal Observatory, this telescope I therefore esteemed to be of nearly equal validity, as to quantity of light, with that at Greenwich, but reduced to the more commodious length of three feet six inches.

* Some observations made therewith, after it was completed, I transmitted to my friend Mr. AUBERT; the consistency of which induced him to procure a similar instrument to be made for *cometary* and such kind of observations as cannot be commodiously made in the meridian. Of this instrument the Rev. Mr. WOLLASTON has made honourable mention in Phil. Trans. Vol. LXXV. for 1785, p. 348.

† The common wire micrometer, as used by Dr. BRADLEY, and described from a paper of the Doctor's hand-writing, is given by Dr. MASKELYNE in the Philosophical Transactions, Vol. LXII. for the year 1772; and in addition to which I must beg leave to observe, that the telescopes then in use for the micro-

ſame time more effectually to anſwer the purpoſe of GRAHAM's *aſtronomical ſector*, which was contrived by him (as Dr. SMITH informs us) to ſupply the deficiencies of the micrometer then in uſe *.

The moſt neceſſary and fundamental improvements ſeemed to be; firſt, that of rendering the micrometer teleſcope manageable upon an equatorial motion; and, ſecondly, the contrivance of a ſtand of ſuch ſolidity and ſtedfaſtneſs that the teleſcope might preſerve the poſition in which it was placed, for a length of time: for it occurred to me, that if the teleſcope could be maintained at reſt; or in a degree of ſtability ſuperior to that of the aſtronomical ſector; then the neceſſity of taking in a greater compaſs in declination than could be commodiouſly given to the field of a teleſcope would be the leſs neceſſary: for, inſtead of confining the object to a compariſon with a ſtar not differing more than a few minutes of time, or at moſt a quarter of an hour in right aſcenſion, thoſe compariſons could be extended to an hour or two, or even on occaſion to three or four hours; there being ſcarcely any part of the heavens ſo devoid of ſtars, of a ſuitable magnitude for theſe obſervations, but that a proper one may be found within that compaſs in right aſcenſion, provided there is allowed

micrometer were from ten to fifteen feet long, made with wooden tubes, ſupported at each end upon two wooden ſupports, by which the teleſcope could be managed in altitude and azimuth; but not to follow a celeſtial object in its proper motion on one center: which apparatus, I believe, is ſtill remaining in the Royal Obſervatory.

* This inſtrument is deſcribed in SMITH's Optics, Vol. II. p. 350.; and the original one, made by Mr. GRAHAM, was, at his death, placed in the Royal Obſervatory, and is mounted upon a three legged ſtand of *wood*.

the difference of a degree, either north or south of the object, in declination *.

Considering, however, that the approaching elongation would be in the morning; and that the best chance of seeing Mercury with this instrument would be some time in the twilight, between Mercury's rising and the rising of the Sun; yet, on supposition of catching the planet in his passage over the wires, there would be no chance of seeing any star pass over the field, wherewith to compare him, till the following evening, which being at least fourteen hours, the certain position of the telescope for so great a length of time was *almost* more than I could reasonably hope for. To judge how far I might form an expectation, by way of a previous trial, I compared Saturn with γ Capricorni †, and found the return

* The field of the telescope of Mr. Ambler's instrument is two degrees; but that of the original, wherewith this observation was made, is 1° 17′: to gain which, the eye-glass being immoveable (and *achromatic* to prevent the indistinctness that would otherwise have taken place near the border) the magnifying power was obliged to be considerably reduced, in respect of what has been usual for micrometers, that is, so as not to exceed 20 times: in consequence, there is, therefore, no need for so long a telescope, this being but 34½ inches focal length of object-glass; but being a double achromatic, made by the late Mr. John Dollond, it is capable of as great an aperture as could be given to the simple object-glasses of twelve or fifteen feet telescopes, that were then generally given to micrometers; but the pencil of light being greatest in this, is attended with this advantage, that the small stars can be seen very distinct and in great abundance, like the modern night-glasses: and there is in reality no need of great magnifying powers for the present purpose; for the place of the wire being viewed by an eye-glass, of about 1¾ inches focus, its place may be distinguished to less than a 2300th part of an inch, which, on the radius of 34½ inches, is scarce 2″½ of a degree; and which, as I apprehend, is nearer the truth than can reasonably be expected from instruments out of the meridian.

† According to this observation ♄ preceded γ ♑ the 2d of Sept. at 9 h. 15′ P.M. mean time by 30′ 9″·7 MT, and with greater declination south than γ by 41′ 23″.

of the ſtar to the ſame place two evenings afterwards, both in right aſcenſion and declination, was ſo near, that I concluded I might very well expect a good obſervation of Mercury, in caſe I could get a ſight of him, though the ſtars wherewith he was to be compared lay at the diſtance of the following evening at the ſooneſt.

The micrometer is furniſhed with five horary wires, denominated in their order *a*, A, B, C, D (B being the middle horary wire), and the two declination wires are denominated *A* and *B*, each moveable by a ſeparate and independent micrometer-ſcrew, from the outſide of the field to the center, and a little beyond it; ſo that each wire can be moved into the place of the other when at or near the center *.

The morning of the 23d of September, about a quarter paſt five o'clock, the air being clear and perfectly ſerene, it being then about an hour after Mercury's riſing, and near three-quarters of an hour before the riſing of the ſun, I very readily found Mercury with the teleſcope, and when found could eaſily ſee him with an opera glaſs; and Mercury being then in a ſtate of very little alteration of declination, I adjuſted one of the declination wires to his apparent run, by making him traverſe the *whole* field. The obſervations were then taken as

* In Dr. BRADLEY's Paper it is ſaid, that before the *late* alterations, both the declination wires were made moveable; and that it was an improvement to make one of them fixed, and one only moveable. But however they might be immediately preceding the Doctor's time, I believe, the original micrometers by Mr. TOWNLEY were with one fixed and one moveable declination wire, as I have ſeen one in this form among the remaining apparatus of Mr. ABRAHAM SHARPE. In an inſtrument, however, fitted up for the purpoſes of the *equatorial micrometer*, I believe, it will be found moſt convenient to have both theſe wires moveable; as by this means they not only are enabled to ſlide into *each other's place*, but every part of the frame of the inſtrument remains fixed during the whole of the obſervation, the two ſlides carrying theſe two wires excepted.

in the first table; and in the evening I was lucky enough to get those of λ Ceti and ο Tauri, intending to repeat the whole the next morning and evening. The next morning proved cloudy, and so continued, that I saw the planet no more; but in the evening of the 26th, I found the stars come again so near the same declination, that I was encouraged to continue the observation to see what change would happen. It then came on bad rainy weather till the 30th, when I again repeated the observation, and found the stars to come so near in declination that I was fully satisfied of the stability of the instrument, so far at least as could regard twenty-four hours: but as I was then appointed to go a journey, and could have no other use for it, I locked the door of the Observatory, leaving the instrument in its position, that I might see what change would happen by the time of my return; and was quite astonished to find, on the 13th of October, that it had remained in a manner unmoved; for it had suffered no more apparent alteration than what might occur by the errors of observing, and alterations of the clocks and transit.

It must, however, be remarked, that, besides that in the construction of the instrument every thing was contrived that appeared likely to give it firmness, it was rested upon the *frustum* of an hexagonal pyramid of stone, in the founding whereof great care was taken as to its solidity, and was detached from the floor for supporting the observer.

This Observatory at *Austhorpe* I esteem in the latitude of 53° 47′ 54″ N. and 5′ 50″ of time W. from *Greenwich*.

Table 1. Observations of Mercury at his elongation Sept. 1786, with an Equatorial Micrometer.

Day, object, and wires.	Hour.	Time as taken by the clock.	Time reduced to min. & Sec.	Reduced to the middle wire.	Mean of the wires.	Parts of the microm.	Micrometer reduc.
Sept. 23.	AM.	M. q. hea.	′ ″	′ ″	′ ″	Rev. Pts.	Rev. Pts.
Merc. to wire *a*	5	24 3 5	24 47.5	26 34.8			
A	—	25 3 14½	25 52.3	26 34.8			
Middle wire B	—	26 2 9½	26 34.7	26 34.7	26 34 7	*B* 28.85	S 0.74
C	—	27 1 6	27 18	26 34.7	N. B. The telescope's center was pointed to horary circle VI. 34½ Decl. N. 7° 48′.		
D	—	28 1 15	28 22.5	26 34.6			
	PM.						
a Ceti to B	9	15 1 27	15 28.5	15 28.5	15 28.4		
C	—	16 0 23	16 11.5	15 28.3			
a Tauri to B	—	40 1 25	40 27.5	40 27.5	40 27.4	*B* 8.39	N19.72
C	—	41 0 21	41 10.5	40 27.3			
Sept. 26.							
a Ceti to *a*	9	2 2 13	2 36.5	4 23.5			
A	—	3 2 23	3 41.5	4 23.9	4 23.8	*B* 16.97	N11.14
C	—	5 0 14½	5 7.3	4 24.1			
a Tauri to A	—	28 2 21	28 40.5	29 22.9			
B	—	29 1 16	29 23	29 23	29 23	*B* 8.47	N19.64
Sept. 30.							
a Ceti to *a*	8	47 0 3½	47 1.8	48 48.8			
A	—	48 0 13	48 6.5	48 48.9			
B	—	48 3 8	48 49	48 49	48 49	*B* 16.97	N11.14
C	—	49 2 4½	49 32.3	48 49.1			
a Tauri to A	9	13 0 11	13 5.5	13 47.9			
B	—	13 3 6	13 48	13 48	13 48.1	*B* 8.48	N19.63
C	—	14 2 3	14 31.5	13 48.3			
a Orionis to *a*	11	41 3 12	41 51	43 38			
A	—	42 3 20½	42 55.3	43 37.7			
B	—	43 2 17	43 38.5	43 38.5	43 37.9	*A* 15.07	S 15.72
C	—	44 1 12	44 21	43 37.8			
D	—	45 1 20½	45 25.3	43 37.7			
Oct. 13.							
a Ceti to A	7	58 3 0	58 45	59 27.4			
B	—	59 1 25½	59 27.7	59 27.7	59 27.5	*B* 16.97	N11.14
C	8	0 0 21½	0 10.7	59 27.5			
a Tauri to C	—	25 0 20	25 10	24 26.8	24 26.8	*B* 8.50	N19.61
a Orionis to *a*	10	52 2 0½	52 30.2	54 17.2			
A	—	53 2 9	53 34.5	54 16.9			
B	—	54 1 4½	54 17.2	54 17.2	54 17.1	*A* 15.07	S 15.77
C	—	55 0 0½	55 0.4	54 17			
D	—	56 0 10	56 5	54 17.4			
1	2	3	4	5	6	7	8

Table II. For reducing the horary wires of the Equatorial Micrometer to that of the middle, when taken in mean ſolar time.

	Wires.	Equatorial object.		Declination 7° 48′	
		⊙'s run	✱'s run	⊙'s run	✱'s run
		′ ″	′ ″	′ ″	′ ″
The 1ſt wire precedes the middle, add	*a*	1 46.2	1 46	1 47.3	1 47
2d —— ——	A	0 42.1	0 42	0 42.5	0 42.4
3d, or middle wire — —	B	— —	— —	— —	— —
4th is ſubſequent to the middle, ſubtract	C	0 42.9	0 42.8	0 43.3	0 43.2
5th —— —— ——	D	1 46.8	1 46.6	1 47.9	1 47.6
1	2	3	4	5	6

Table

Table III. Containing the obſervations of Tab. I. reduced ſo as to ſhew the correct differences of right aſcenſion and declination between Mercury and the ſtars wherewith he was compared.

1786. Date and object.	Hour.	Paſſage over mid. hor. wire by journ. clock.	Correction to reduce the clock to mean time.	Correct mean time of the obſervation.	Intervals of mean-time of different obſervations.	Parts of micrometer from the teleſcope's center.	Pts. of micr. reduced = declin. from the teleſcope's center.
Sept. 23.	AM	′ ″	′ ″	h. ′ ″		Rev. Pts.	S ′ ″
☿ to the mid. wire	5	26 34.7	−3 59.8	5 22 34.9		S 0.74	S 1 8
	PM				h. ′ ″		
λ Ceti to mid. wire	9	15 28.4	−4 0.1	9 11 28.3	15 48 53.4		
ρ Tauri to the ſame	9	40 27.4	−4 0.1	9 36 27.3	0 24 59	N19.72	N 30 26
Sept. 26.							
λ Ceti to mid. wire	9	4 23.8	−4 43.2	8 59 40.6		N11.14	N 17 11
ρ Tauri to the ſame	9	29 23	−4 43.2	9 24 39.8	0 24 59.2	N19.64	N 30 18
Sept 30.							
λ Ceti to mid. wire	8	48 49	−4 50.9	8 43 58.1		N11.14	N 17 11
ρ Tauri to the ſame	9	13 48.1	−4 50.9	9 8 57.2	0 24 59.1	N19.63	N 30 17
α Orion. to the ſame	11	43 37.9	−4 50.8	11 38 47.1	2 29 49.9	S 15.77	S 24 20
Oct. 13.							
λ Ceti to mid wire	7	59 27.5	−6 36.4	7 52 51.1		N11.14	N 17 11
ρ Tauri to the ſame	8	24 26.8	−6 36.8	8 17 50	0 24 58.9	N19.61	N 30 15
α Orion. to the ſame	10	54 17.1	−6 37	10 47 40.1	2 29 50.1	S 15.77	S 24 20
1	2	3	4	5	6	7	8

Table

Table IV. Deviations in the direction of the axis of the telescope of the Equatorial Micrometer in right ascension and declination in 20 days, from the 23d of September to the 13th of October, both inclusive.

Objects observed.		3 days from the 23d to 26th.	4 days from the 26th to 30th.	7 days from the 23d to 30th.	13 days from the 30th to 13th.	17 days from the 26th to 13th.	20 days from the 23d to 13th.
λ Ceti	R. Ascension	exact 0″	too late 1.1″	too late 1.1″	too soon 0.3″	too late 0.8″	too late 0.8″
	Declination	not taken the 23d	exact 0.	not taken the 23d	exact 0.	exact 0.	not taken the 23d
ρ Tauri	R. Ascension	too late 0.2	too late 1.0	too late 1.2	too soon 0.5	too late 0.5	too late 0.7
	Declination	South 8.	South 1.	South 9.	South 2.	South 3.	South 11.
ω Orionis	R. Ascension	– – –	– – –	– – –	too soon 0.3	– – –	– – –
	Declination	– – –	– – –	– – –	exact 0.	– – –	– – –

N. B. The right ascension is expressed in the integer and decimal parts of a second of time. The declination is expressed in seconds of a degree.

Explanation of the leſs obvious parts of the Tables of the Obſervation of Mercury *near his Elongation*, Sept. 1786.

The third column of Tab. I. contains the times of obſervation as they were taken down from the half-ſecond *journeyman* clock, in minutes, quarters, and *beats*, according to the following method; which was, by taking up the beat when the ſecond hand came to 15, 30, 45, or 60, and then counting 30 beats repeatedly till the arrival of the object at the middle of the wire it was approaching; after its arrival, the beats (or interval between two beats) being retained in memory, and the eye caſt upon the dial-plate, it was eaſily ſeen whether it was ſo many beats more than the quarter, the half, three-quarters, or the whole minute, and was ſet down accordingly. Thoſe reduced to minutes, ſeconds, and tenths of ſeconds, by allowing .2 or .3 for the quarter ſecond, .5 the half, and .7 or .8 for the three-quarters of a ſecond, are contained in the fourth column. The reduction of the fourth column to the fifth was by means of the auxiliary Tab. II.; and Mercury being then nearly ſtationary reſpecting the ſun, the ſun's run was uſed for the planet inſtead of that of a ſtar. The mean of each ſet of obſervations of the fifth column is carried into the ſixth.

The ſeventh column contains the parts of the micrometer as they were read off; to render which intelligible, it is to be noted, that the declination wire *A* travels from the upper ſide of the field of view of the teleſcope towards the center, and ſomewhat beyond it: and upon it are taken all the objects that paſs the field of view on the upper ſide, anſwerable (by inverſion of the object) to the ſouthern half of the field: and in like manner thoſe that paſs the field of view on the lower

half are taken upon the wire *B*, and for the ſame reaſon denote a declination north. The ſcale of the micrometers of each wire begins from a point aſſumed ſomewhat without the field, and the number increaſes from thence towards the center of the field, and continues beyond it; the integral parts are the turns of the ſcrew, and the centeſimal the diviſions of the index plate, being divided into 100 parts. The point of the ſcale, anſwerable to the center of the field of view, having been found by obſervations on each ſcale reſpectively; when the wire *A (Auſtralis)* ſtands at 30.84, it is in the center of the field; and when the wire *B (Borealis)* is at 28.11, it alſo cuts the ſame center. Hence the parts of the micrometer being reſpectively taken from thoſe two numbers (which may therefore be called *conſtant* numbers) the remainder will be the diſtance of each reſpective wire from the center in parts of the micrometer. Thus, in the obſervation of *o* Tauri upon the 23d, the parts are *B* 8.39; this taken from 28.11, leaves N 19.72, which are placed in col. 8. as the diſtance, in parts of the micrometer, that *o* Tauri paſſed north of the center of the field of view, or axis of the teleſcope.

In like manner, in the obſervation of Mercury on the 23d, the parts are *B* 28.85; but this being greater than the conſtant number 28.11, the exceſs will be .74 parts; which being the parts reaching beyond the center, they will be ſo much *ſouth* of it, and are ſet down therefore in col. 8. S 0.74: and in this manner the declinations of the reſt are made out, from their reſpective numbers of parts of the micrometer, and ſet down in col. 8.

The numbers of the ſixth column of Tab. I. are tranſferred to the third column of Tab. II.; and the declinations ſet down in parts of the micrometer, Tab. I. col. 8. are tranſferred to col. 7. of Tab. III.

Col. 4. of this table contains the corrections of the times deduced from the journeyman clock (as *per* col. 3.) to reduce it to mean time; which corrections are made out from the general account of the goings of the tranſit clock, corrected by tranſits of the ſun, taken the 22d, 23d, 27th, and 30th of September, and the 12th, 13th, and 14th of October *. The journeyman clock was regularly compared at nights and mornings with the tranſit clock; and generally immediately after the obſervation. The *meridian* and *rotative* obſervatories in which the clocks reſpectively were, are at the diſtance of 53 yards E. and W.; the compariſons were made by a ſeconds ſtop watch †.

The numbers of the fourth column being properly applied to thoſe of the third produce the fifth; and which, with the ſixth column, will be ſufficiently explained by their titles. The parts of the micrometer in the ſeventh column, being reduced into minutes and ſeconds, are contained in col. 8. and reſpectively ſhew the minutes and ſeconds at which each object paſſed to the north or ſouth of the center of the teleſcope. The value of the parts of the micrometer were obtained by previous obſervations, from whence the following rule was deduced: the numbers of turns and centeſimal parts being conſidered as integral, and divided by 1.08, the quotient will be the number of ſeconds. Thus, in the obſervation of *o* Tauri

* The tranſit clock was made by HINDLEY, and has a pendulum rod of *cedar* wood.

† The journeyman clock was generally ſet to the tranſit clock on Sunday mornings; and when from home the former was ſuffered to go down. The journeyman will *generally* agree with the tranſit clock to 2″ in 24 hours; but during the period of theſe obſervations, went remarkably well.

upon

upon the 23d, the parts 1972, divided by 1.08, gives 1826″ = 30′ 26″; and the parts of mercury .74, divided by 1.08 = 68″ = 1′ 8″. Now the teleſcope being fixed to one point of the heavens during the whole period of theſe obſervations, without any motion of any of the parts, the ſcrews commanding the declination wires *A* and *B* excepted, we are enabled to judge of its ſteadineſs to this point by the following remarks. If it varied in declination, this would be ſhewn by the paſſage of the ſame ſtar at a different diſtance from the center of the teleſcope at different revolutions; and if it varied in right aſcenſion, it would be ſhewn by its not paſſing the horary wires at the due time, according to the acceleration of the ſtars upon the mean time of the ſun. Both the right aſcenſion and declination may be varied by differences of refraction of the air at the ſame altitude; and the right aſcenſion is further liable to be *apparently* varied, by the errors of the tranſit inſtrument, the tranſit clock, the transferring of its time to the journeyman clock, the intermediate errors of the ſame, and of the obſervation itſelf; and as there paſſed an interval of almoſt 16 hours betwixt the paſſage of Mercury over the field of view of the teleſcope and that of λ Ceti, which was the neareſt ſtar wherewith a compariſon could be made, it will be a ſatisfaction to ſee, as before intimated, what variations aroſe in ſtill greater intervals of time.

In right aſcenſion.

	h.	′	″
Thus λ Ceti upon Sept. 23. paſſed the horary wires at —	9	11	28.3
and ——— 26. ———	8	59	40.6
λ Ceti therefore came ſooner in three days by —		11	47.7
but ——— ought to accelerate on mean time — —		11	47.7
——— therefore came after three days exactly to the time,			

Again,

	h. ′ ″
Again, ϑ Tauri upon Sept. 25. paſſed the horary wire at	9 36 27.8
and — 26. —	9 24 39.8
ϑ Tauri therefore came ſooner after three days by	11 47.5
— ought to accelerate on mean time	11 47.7
— therefore came too late in three days by	.2

In declination.

	° ′ ″
ϑ Tauri upon Sept. 25. paſſed north of teleſcope's center	30 26
— 26. —	30 18
— therefore paſſed leſs north, or more ſouth, than before by	8

In like manner every compariſon that Tab. III. affords is particularly ſet down in Tab. IV. which containing thirteen compariſons in right aſcenſion and ten in declination, the greateſt deviation in right aſcenſion is 1″.2, and 11″ of a degree in declination. This ſuppoſes every error before mentioned to reſide in the inſtrument, and every other inſtrument and obſervation, which were concerned in the reſult, to be perfect; which, from the ſmallneſs of the total errors, ſeems to indicate a degree of ſteadineſs in the inſtrument unexperienced or unnoticed before.

Deduction of the poſition of Mercury *from the preceding obſervations as ſet down in* Tab. III.

In right aſcenſion from col. 6.

	′ ″
ϑ Tauri followed λ Ceti Sept. 23.	24 59
— 26.	24 59.2
— 30.	24 59.1
— Oct. 13.	24 58.7
— at a mean of the four	24 59

a Orionis

	h.	′	″
α Orionis followed ρ Tauri Sept. 30.	2	29	49.9
—— Oct. 13.	2	29	50.1
—— at a mean	2	29	50
Now Mercury preceded λ Ceti Sept. 23.	15	48	53.4
λ Ceti preceded ρ Tauri by mean of four		24	59
ρ Tauri preceded α Orionis by mean of two	2	29	50
Mercury therefore preceded α Orionis by	18	43	42.4

In declination from col. 8.

	°	′	″
Sept. 23., A.M. Mercury passed the middle horary wire, south of its center		1	8
Same evening ρ Tauri passed the middle horary wire, north of it		30	26
Therefore Mercury passed the middle horary wire more S. than ρ Tauri by		31	34

But Sept. 26. λ Ceti passed N. of center	17	11	Diff.	13 7
—— ρ Tauri	30	18		
—— 30. λ Ceti	17	21	——	13 6
—— ρ Tauri	30	17		
—— Oct. 13. λ Ceti	17	11	——	13 4
—— ρ Tauri	30	15		

From the smallness of the above differences we may infer, that very little uncertainty in declination had attended the passage of ρ Tauri upon Sept. 23.

Upon Sept. 30. ρ Tauri passed N.	30	17	Sum	54	37
—— α Orionis —— S.	24	20			
Upon Oct. 13. ρ Tauri passed N.	30	15	——	54	35
—— α Orionis —— S.	24	20			
α Orionis then at a mean passed more south than ρ Tauri			——	54	36
Merc. therefore on the 23d passed with more N. declination than α Orionis				23	2

Investigation of the effects of refraction.

The preceding deductions and remarks shew the consistency of the observations with themselves; yet, from the position of the telescope, it being only elevated 11°½ above the horizon *, it is necessary to examine how far the deductions above specified were capable of being affected by refraction. And in this respect it will appear, that if it be supposed, there is no difference in the quantity of refraction of such objects as appear within the limits of the field of view of this instrument (which is 1° 17′), then their relative positions to each other will not be affected thereby: for if in fig. 1. (Tab. XIII.) we suppose the circle VHRO to represent the boundary of the field of view, HO being an horizontal and VR a vertical line, each passing through the center of the field at L; and if PLP denotes a part of a parallel of declination, then BLX perpendicular thereto, will be a part of an horary circle, both passing through the same center. Now let $d*$ be the apparent path of a star, supposing it unaffected by refraction till it comes to the vertical line at *, and there to be lifted up by refraction in the said vertical to L. Let $e+$ denote another star, also unaffected by refraction, to pass along the different parallel of declination $e+$ till it comes to +; then, if it be supposed that the two stars are both situated in the same horary circle, if at the point + refraction takes place, and by hypothesis this is lifted up equally with the other, in the perpendicular $+l$, then the line $+*$ being drawn through the places of the two stars, will be cotemporary and parallel to LX; and the figure $l+*L$ being evidently a rhomboides, the two stars, so altered by

* This will readily be deduced by inspection of the celestial globe.

refraction,

refraction, will arrive together at the horary circle LX at the same time, and with the same difference of declination, as if no refraction had taken place. It is therefore only the *difference* of refraction which takes place in objects at different heights in the *same field*, that can alter their relative situations: however, it appears necessary to examine what this may amount to.

Let the letters in fig. 2. denote the same things as before; to which we will add, that *a*, A, B, C, D, denote the parallel horary wires of the micrometer, and *AA*, *BB*, the declination wires, denoted *A* and *B* in the tables: now from the celestial globe we shall also readily obtain the horary angle VLP = 54°½ = L*bc*. Let now an object pass along the wire *AA* from the horizontal line at *d* to the vertical line at *b*; in this it will pass through a difference of refraction, according as it gets more and more elevated above the horizontal line HO; and let the elevation L*b* be half a degree or 30 minutes: then, according to Dr. BRADLEY's Table of Refraction *, the difference of refraction betwixt the 78th and 79th degrees of zenith distance is 23″.6, half of which 11″.8, may be esteemed the difference of refraction for a difference of half a degree of altitude at 78°½ zenith distance, or of 11°½ altitude: the object, therefore, in passing from the horizontal line at *d* to the vertical line at *b* passes through every difference of refraction from 0″ to 11″.8; and the question is, how much it is at a *medium*, that is, when it arrives at the middle wire at the point *c*? From this point let fall the perpendicular *ce*. Now, the proportion of the sides of the triangle *db*L being given from construction, they may be taken off by a scale, *viz.*

* Inserted in Dr. MASKELYNE's Observations, Vol. I. p. 15.

Suppoſe $Lb = 174$
$db = 299$
$dL = 242$
and aſſuming the ſide $Lb = 30$
the other ſides by proportion as above will be $\begin{cases} db = 51.6 \\ dL = 41.7 \end{cases}$

The triangles Lbc and dce are ſimilar to dbL; therefore ſay, as $db = 51.6 : dL = 41.7 :: Lb = 30 : Lc = 24$; and as $Lb = 30 : Lc = 24 :: dL = 41.7 : dc = 33.5$; and again, as $db = 51.6 : dc = 33.5 :: Lb = 30 : ce = 19.5$: but this will affect the declination, only in proportion of the line ef drawn parallel to LX; and it will affect the right aſcenſion according to the line fc: but the triangle ecf being ſimilar to the original one dbL, we ſhall have $db = 51.6 : Lb = 30 :: ce = 19.5 : fc = 11.3$ for the line affecting the right aſcenſion; and alſo, as $db = 51.6 : dL = 41.7 :: ce = 19.5 : ef = 15.8$ for the line affecting the declination. But the effect of difference of refraction upon the line $Lb = 30'$ being only $11''.8$, the reſpective effects of the lines fc and ef will be in proportion; that is,

as $30' : 11'.3 :: 11''.8 : 4''.4$ for the effect in right aſcenſion,
and as $30 : 15.8 :: 11.8 : 6.2$ ——————— declination;
but as it has been determined, that when the line Lb is 30 minutes, the line LC, or the correſponding declination, will be only 24 minutes; the effects of refraction above ſtated will be therefore due to $24'$.

Correction for the poſition of the wires.

The above corrections take place on ſuppoſition that the ſeveral wires of the micrometer were ſtrictly parallel to the reſpective parts of the circles of declination, and horary circles in the heavens; but in the practical uſe of this inſtrument it

is

is found more convenient, on account of a ready and certain adjuſtment, to place one of the wires *AA* or *BB* parallel to the apparent track of the ſtar wherewith the *planetary* body is to be compared: in conſequence, when the ſtar *, fig. 1. is lifted up to L, it will not ſtrictly purſue the line LP; but being leſs and leſs lifted up as it mounts higher, it will apparently fall more and more below the line LP as it aſcends above the line HO, and will therefore take a courſe, ſuppoſe L*ρ*. The wire PLP being therefore adjuſted to agree with *p*L*ρ*; by conſtruction of the inſtrument, the wire BLX will aſſume the poſition *q*L*x* perpendicular to *p*L*ρ*. The ſtar, therefore, that ran along the parallel *e*+ before it ſuffered refraction, and at + was ſuppoſed to be lifted up to *l*, there not meeting LX will take the courſe *ly*, nearly parallel to L*ρ*, and have ſome diſtance, as *lz*, to travel before it arrives at the new-placed wire L*x*; and it is now proper to examine what this quantity may be.

Through the point *z* draw the line *rzos* parallel to HO, and cutting the vertical RLV in *o*, and let L*ρ* be aſſumed = 30″; then, ſince the angle XL*x* is ſuppoſed to be *minute*, the *groſs* proportions of the ſides of the triangles L*yz* and *Lyl* may be, for this purpoſe, ſuppoſed the ſame *, and the ſame as L*bc*, *dbL*, fig. 2. to which the triangle L*zo*, fig. 1. will alſo be ſimilar; as likewiſe the triangle *yzo*, and alſo the little triangle *zlv*: but making the ſide *lv* of the triangle *zlv* equal to the effect of refraction in perpendicular = 11″.8; then, to find the ſide *lz*,

* I am aware, that the ſuppoſition of the ſides of the triangles L*yz* and L*yl* being the ſame cannot be ſtrictly ſo; nor can they have the ſame proportions; nor are any of the lines concerned right lines, that are ſuppoſed ſuch; but aſſumptions *near* the truth are allowable for the correction of an error in the *greateſt part*, that if uncorrected would ſcarcely amount to a *groſs error*.

the diſtance run from the firſt to the laſt ſuppoſed place of the wire, we need only ſay, as $Lb = 30 : db = 51.6 :: bv = 11'.8 : bx = 20''.3$; and this will be its value when the declination Lc, fig. 2. is 24′; but then the declination L*l* or Lα, fig. 1. being greater than the perpendicular ſide L*o* (aſſumed 30′) in the proportion of Lα : L*o*, ſay, by ſimilarity of triangles converſely, as $dL = 41.7 : db = 51.6 :: Lo = 30' : L\alpha = 38'.2$; but as the correction before ſtated of 20″.3 is an angular error, taking place in proportion to the diſtance from the center, or the declination: for the declination given of 24′ ſay, as 38.2 : 24′ :: 20″.3 : 13: to which adding 4″.4, we ſhall have 17″.4 for the whole error in right aſcenſion, ſuppoſing it in the equator, but muſt be again increaſed in the proportion in which a ſtar having declination is ſlower than a ſtar in the equator; that is, it muſt be increaſed in the proportion of any of the numbers in the fourth column of Tab. II. to the ſimilar ones in col. 6. of the ſame table; that is, as 1′ 46″ : 1′ 47″ or as 106 : 107, :: 17″.4 : 17″6 *.

As all theſe errors, ariſing from difference of refraction, are in proportion of the diſtance of the object from the center of the teleſcope, they will take place in proportion to the difference of declination of the two objects to be compared, whether they have paſſed the field on the ſame, or on different ſides of the center. Now the difference of declination of Mercury and α Orionis being only 23′ 2″, and the quantities being made out for 24′ ſay (rejecting the 2 ſeconds), as 24′ : 23′ :: 17″.4 : 16″.7, which turned into time in the run of the ſtar will be 1″.1 in right aſcenſion.

* My friend Dr. MASKELYNE obſerves, that in *ſtricteſt* each ſtar ought to have its own proper reduction, on account of *difference* of declination, which in *extreme caſes* will amount to a ſenſible quantity.

Say again, as 24′ : 23′ :: 6″.2 : 6″, the correction in declination. From the near equality of the lines L*l* and L*z*, it is evident, that no correction of *declination* is neceſſary on account of the inclination of the wires, the whole difference falling in right aſcenſion. As therefore Mercury paſſed with 23′ 2″ more north declination than α Orionis, and paſſed through a part of the *medium* that lifted him up leſs; it therefore gave him leſs north declination than it did to α, and therefore apparently diminiſhed the real difference; hence 6″ muſt be added to the apparent difference 23′ 2″, making it 23′ 8″ difference of declination: and as Mercury was lifted up leſs than α, he would not ſo ſoon come to the middle wire by 1″.1 as he ſhould have done, he therefore came too late by 1″.1, which muſt be ſubtracted from the time of Mercury's paſſage the 2d of Sept. which will increaſe the time in which he preceded α Orionis; that is, 18 h. 43′ 42″.4 increaſed by 1.1 will become 18 h. 43′ 43″.5 difference of right aſcenſion.

I have been the more particular in the inveſtigation of this obſervation, firſt of all to aſcertain the degree of dependance that may be formed on an inſtrument of the kind; and, ſecondly, to infer ſuch eaſy and ſimple rules, that other ſimilar obſervations may be the more eaſily reduced. Being therefore ſatisfied of the ſtability of the inſtrument; if we had concluded the obſervation with that of Mercury in the morning, and of ε Tauri in the evening of the 23d, then the reſult from Tab. III. ſhould have been

	h.	′	″
Mercury paſſed the wires at	17	22	34.9
And ε Tauri paſſed at	9	36	27.3
Difference of right aſcenſion	15	13	52.4

which

which is the very ſame as was before deduced from the mean of the whole:

				′	″
And if to Mercury's declination ſouth of teleſcope's center			–	1	8
We add *o* Tauri's	north		–	30	26
We ſhall have for the difference of declination		–	–	31	34

the ſame as before determined. Our obſervation would, therefore, in this caſe ſimply have been, that Mercury preceded *o* Tauri in right aſcenſion 15 h. 13′ 52″.4 mean time, and paſſed the wire with more ſouth declination than *o* Tauri by 31′ 34″.

After this, *o* Tauri would have required to be compared with ſome well rectified ſtar by meridian inſtruments; but in the preſent caſe *α* Orionis, one of Dr. MASKELYNE's Catalogue of 34 principal ſtars happened to lie ſufficiently near the ſame parallel of declination, to admit of *o* Tauri to be compared therewith by the ſame inſtrument, while pointed to the ſame place of the heavens. The operations which were ſubſequent, therefore, muſt be conſidered as intended to ſave thoſe of a meridian inſtrument.

Now had our obſervation concluded with the above, then the correction would have taken place upon the difference of declination of *o* Tauri with Mercury, inſtead of the ultimate one with *α* Orionis; but it muſt be obſerved, that whatever quantity of correction the difference of declination would occaſion, it would be compenſated in the difference of refraction of of *o* Tauri and *α* Orionis, when they came to be obſerved on the meridian; however, in the preſent caſe it happens to be more commodious, as both can be done under one.

Preparatory then to the laying down the ſimple rule for the correction of refraction, it is proper to premiſe, that it is evi-

dent,

dent, the lines, fig. 2. Lb, Lc, ce, ef, being in continued proportion Lb will be to ef in triplicate proportion of Lb to Lc; and that Lc will be to ef in duplicate proportion of $Lb : Lc$. The difference of declination, therefore, due to 30′ difference of elevation will be as Lb to Lc simply; but the effect of difference of refraction in declination will be less than the difference of declination in the proportion of $Lb^2 : Lc^2$; and that the effect of difference of refraction in right ascension will be less than the difference of refraction in declination in the proportion of $Lb : cb$ simply.

Now it has been remarked, that the elevation of the telescope's center above the horizon, and the horary angle VLP, will always be readily given near enough for the purpose by the globe. A triangle given Lbd can therefore be constructed, and the side Lb being made 30′ (or any convenient aliquot part of a degree) the other sides will be found by proportion: say then, as in the present case, $db = 51.6 : dL = 41.7 :: Lb = 30 : Lc = 24$, for the difference of declination corresponding to half a degree of altitude: say then, as $51.6^2 : 41.7^2$, that is, as $2663 : 1739 :: 24 : 15.7 = ef$. But without troubling ourselves with high numbers, if we take the proportion 51.6 to 41.7 by the slide-rule *twice*, we shall arrive at 15.7, near enough for the value of the line ef: say then, as $Lb = 30 : ef = 15.7 :: 11''.8 : 6''.2$ for the refraction in declination: and as $dL = 41.7 : Lb = 30 :: 6''.2 : 4''.4$ for the refraction in right ascension, according to the *true* position of the wires: and, for the correction of right ascension in the position of the wires, say,

Fig. 2. Fig 1.

As $Lb = 30 : db = 51.6 :: lv = 11''.8 : lx = 20''.3$;

and again, $dL = 41.7 : db = 51.6 :: Lo = 30' : La = 38'.2$.

Take

Take now L*b*, fig. 1. = L*c*, fig. 2. = 24′, and draw the line *bik*, fig. 1. parallel to the line *lxy*, and then ſay, as L*x* = 38′.2 : L*b* (or L*i*) = 24′ :: *lz* = 20″.3 : *bi* = 13″, which + 4″.4 = 17″.4 for the whole error in right aſcenſion, with a declination or diſtance from the center of 24′; but as the errors both of right aſcenſion and declination are in proportion to diſtance from the center, as the difference of the planet and ſtar is only 23′, ſay, as 24′ : 23′ :: 17″.4 : 16″.7 = 1″.1 time; and for the declination, ſay again, as 24′ : 23′ :: 6″.2 : 6″ declination *.

Reduction of Mercury's *compariſon with* α Orionis, *to right aſcenſion and declination.*

We have laid it down, that the 23d Sept. 1786, A.M. at 5 h. 22′ 34″.9 mean time, Mercury preceded α Orionis 18 h. 43′ 43″.5, and had then a more northern declination by 23′ 8″.

According to Dr. MASKELYNE's Catalogue of 34 ſtars, the right aſcenſion of α Orionis reduced to the time when he was obſerved is 85° 54′ 12″.

Now as the whole circle of the ſphere makes a revolution in the time that α Orionis makes one turn, which is

h.	′	″	
23	56	4.1	then from this deduct
18	43	43.5	
5	12	20.6	remains for the time that α Orionis preceded

* If the compariſon had been with ο Tauri, then we muſt have ſaid,

As 24′ : 31′½ :: 17″.5 : 23″ = 1″.5 of time,
and 24′ : 31′½ :: 6.2 : 8.1 correction for declination.

N. B. All theſe and the above proportions will be commodiouſly wrought with the ſlide-rule.

Mercury

Fig. 2.

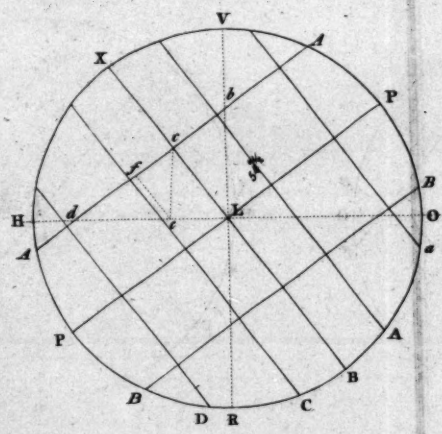
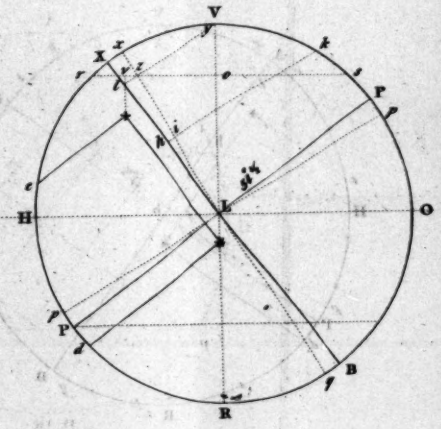


Fig. 1.



Mercury in right ascension; but if α ran the whole rotation $= 360°$ in 23 h. 56′ 4″.1, what portion of it will be run in 5 h. 12′ 20″.6 = 18740″.6?

But 24 h. = 86,400 seconds, and 360 = 1,296,000 seconds:

Time.	Time.	Of degrees.	Of degrees	°	′	″
Say then, as 86400″ : 18740″.6 :: 1296000″ : 281109″ =			–	78	5	9
But, according to Dr. Maskelyne's select Catalogue, the right ascension of α Orionis for Sept. 30, 1786, was, (which add)			–	85	54	12
The right ascension of Mercury at the time of observation was therefore				163	59	21

	°	′	″
According to Dr. Maskelyne's select Catalogue α Orionis had declination north, corrected for precession – –	7	21	8.8
The sum of aberration and nutation from *Connoissance des Temps* –	+		8.4
The correct declination north of α Orionis – –	7	21	17.2
To which add that Mercury passed more north – –		23	8
Mercury's declination therefore was – – –	7	44	25.2

The result.

1786, Sept 23. A.M. at 5 h. 22′ 35″ M.T.	Mercury's	right ascension	–	163	59 21
		declination north	–	7	44 25

XXXIV. *A remarkable Caſe of numerous Births, with Obſervations.* By Maxwell Garthſhore, *M. D. F. R. S. and A. S. in a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read June 21, 1787.

TO SIR JOSEPH BANKS, BART. P. R. S.

SIR, St. Martin's-Lane. May 28, 1787.

THE following very extraordinary caſe, communicated to me by Dr. BLANE, F. R. S. I take the liberty, at his deſire, to tranſmit to you, with his letter to me, containing the proofs of its authenticity; hoping that it will appear to you, as it did to us, worthy of being read at one of the meetings of the Royal Society, as a fact in natural hiſtory, which is equally uncommon, curious, and well vouched. In order, however, to make its ſingularity more apparent, I have taken the liberty to ſubjoin ſome obſervations on births of this kind, with ſuch well authenticated accounts of ſimilar events as I have been able to procure, confining myſelf chiefly to thoſe which have happened in our own country, where we are leaſt likely to be deceived.

I have the honour to be, &c.

MAXWELL GARTHSHORE.

P. S. As one proof of its ſingularity, I, many months ago, employed various friends at Peterſburg, Berlin, Vienna, Lyons, Paris, and Ghent, to collect for me well authenticated caſes of this kind, and I have not as yet been able to procure any.

Copy of a letter from Dr. BLANE, Phyſician to his Majeſty's Navy and to St. Thomas's Hoſpital, F. R. S. to Dr. Garthſhore, Phyſician to the Britiſh Lying-in Hoſpital.

DEAR SIR, Sackville-Street, June 22, 1786.

A few days ago, I received from the country an account of a woman who was delivered of five children at a birth in April laſt. As your extenſive experience and reading in this line of practice enable you to judge, how far this fact is rare or intereſting, I ſubmit it to you, whether it deſerves to be communicated to the Royal Society. Mr. HULL, the gentleman who ſent me the caſe, is a very ſenſible and ingenious practitioner of phyſic at Blackburn, in Lancaſhire. He attended the labour himſelf from beginning to end, and his character for fidelity and accuracy is well known to me, as he was formerly a pupil at the hoſpital to which I am phyſician; ſo that no fact can be better authenticated. He mentions alſo, that he has preſerved all thoſe five children in ſpirits; and, if deſired, he will ſend them for the inſpection of the Society *.

I am, with great regard, &c.

GILBERT BLANE.

* They were accordingly ſent; and having been exhibited to the Society when this Paper was read, are now depoſited in the Muſeum of Mr. JOHN HUNTER.

MARGARET WADDINGTON, aged twenty-one, a poor woman of the townſhip of Lower Darwin, near Blackburn in Lancaſhire, formerly delivered of one child at the full term of pregnancy, conceived a ſecond time about the beginning of December 1785, and from that period became affected with the uſual ſymptoms that attend breeding. At the end of the firſt month, ſhe became lame, complained of conſiderable pains in her loins, and the enlargement of her body was ſo remarkably rapid, that ſhe was then judged by her neighbours to be almoſt half gone with child. At the end of the ſecond month ſhe found herſelf ſomewhat larger, and her breeding complaints continued to increaſe. When the third month was completed, ſhe thought herſelf fully as large as ſhe had formerly been in her ninth month, and to her former ſymptoms of naufea, vomiting, lameneſs, and pain of the loins, ſhe had now added a diſtreſſing ſhortneſs of breath. She continued to increaſe ſo rapidly in ſize, that ſhe thought ſhe could perceive herſelf growing larger every day, and ſhe was under the frequent neceſſity of widening her cloaths. When ſhe reckoned herſelf eighteen weeks gone, ſhe firſt perceived ſomewhat indiſtinctly the motion of a child. By the 20th of April, 1786, all her complaints were become much more diſtreſſing; ſhe had much tenſion and pain over all the abdomen, her vomiting was inceſſant, and ſhe now could not make water but with the utmoſt difficulty. The ſymptoms being palliated by Mr. LANCASTER, ſhe advanced in her pregnancy to Monday the 24th of April, when being ſuppoſed to have arrived at the twentieth week, ſhe was ſeized with labour pains. Theſe continued gradually to increaſe till the next day, about two in the afternoon; at which time I was ſent for, Mr. LANCASTER being abſent,

and

and she was soon delivered of a small, dead, but not putrid, female child. The pains continuing, this was soon followed by a second less child; to this very soon succeeded a third, larger than the first, which was alive; to these a fourth soon followed, somewhat larger than the first, and very putrid; last of all, there soon succeeded a fifth child, larger than any of the former, and born alive. These five children were all females; two were born alive; and the whole operation was performed in the space of fifty minutes. The first made its appearance at two in the afternoon, and the last at ten minutes before three. Each child presented naturally, was preceded by a separate burst of water, and was delivered by the natural pains only. In a short time after the birth of the last, the placenta was expelled by nature without any hæmorrhage, was uncommonly large, and in some places beginning to be putrid. It consisted of one uniform continued cake, and was not divided into distinct placentulæ, the lobulated appearance being nearly equal all over. Each funis was contained in a separate cell, within which each child had been lodged; and it was easy to perceive, by the state of the funis, and that part of the placenta to which it adhered, in which sac the dead, and in which the living children had been contained. I examined the septa of the cells very carefully, but could not divide them as usual into distinct laminæ, nor determine which was chorion or which amnios. I could not prevail on the good women to allow me to carry it home, to be more narrowly inspected; and I submitted more readily to their prejudice for its being burned, as its very soft texture seemed to me to render it hardly capable to bear injection. The two living children having survived their birth but a short time, I was allowed to carry them home; and I have preserved the whole five in spirits, and have

since

ſince weighed and meaſured them, and find their proportions to be as follows in Avoirdupois weight, inches and parts.

	Oz.	Dr.		Inches.
The 1ſt born dead - -	6	12	Length	9
The 2d —— putrid -	4	6		8¼
The 3d —— alive - -	8	12		9½
The 4th —— putrid -	6	12		9½
The 5th —— alive - -	9	—		9½

The mother, in ſpite of the crowds with which her chamber was continually filled, continued to recover, and was able to be out of bed on the 27th and 28th, her third and fourth days; but finding herſelf then weak, by my advice, kept her bed till the 11th of May, when ſhe went out of doors, and on the 21ſt walked to Blackburn, two miles diſtant. This was the 27th day from her delivery, ſhe having entirely recovered her ſtrength without any accident. It may not be improper to add, that the huſband of this woman has been in an infirm ſtate of health for three years paſt, and is now labouring under a confirmed phthiſis.

I am, &c.

Signed, JOHN HULL.

Blackburn, Lancaſhire,
June 9, 1786.

Obſervations

Obſervations on numerous Births.

THOUGH the females of the human ſpecies produce moſt commonly but one child at a birth; and though their formation with only two breaſts, and one nipple to each, renders it probable they were not originally intended to produce in general more than two; yet, from what we know of the womb and its appendages, and what from the lateſt experiments we are led to conjecture as to the mode of conception, we cannot preſume *à priori* to ſet limits to the fertility of nature, nor determine deciſively what number of fœtuſes may be conceived and nouriſhed to a certain period in the human uterus at the ſame time.

The preſent ſingular and well-atteſted caſe aſſures us, that five have certainly been born at once, and we have no title abſolutely to reject all the teſtimonies of even more numerous births, or to ſay that, in ſome rare inſtances, this number has never been exceeded.

What has tended to render relations of this ſort ridiculous, and to throw a degree of diſcredit on the whole, is the many marvellous, and evidently abſurd and incredible hiſtories, which not only the retailers of prodigies, but even the credulous writers of medical obſervations, have collected.

I need only refer thoſe, who wiſh to amuſe themſelves with ſurpriſing relations of this kind, to the curious collections of SCHENKIUS, SCHURIGIUS, AMBROSE PAREY, and others.

But, in order to ſhew how very uncommon births of this kind are, and how truly ſingular the caſe communicated by Mr. HULL to Dr. BLANE is, I take the liberty to ſubjoin a

ſhort

ſhort view of the uſual courſe of nature in this matter among our own country-women, where we are leaſt likely to be deceived.

Though female fertility certainly varies according to the climate, ſituation, and manner of life; yet, I believe, it may be taken for a general rule, that where people live in the moſt ſimple and natural ſtate, if they are the beſt nouriſhed, and if they enjoy the firmeſt health and ſtrength, they will there be the moſt fertile in healthy children; but we have no *data* to determine that they will there have the greateſt number at one birth.

At the Britiſh Lying-in Hoſpital, where we have had 18,300 delivered, the proportion of twins born has been only one in 91 births. In the Weſtminſter Diſpenſary, of 1897 women delivered, the proportion of twins has been once in 80 births; but in the Dublin Lying-in Hoſpital, where above 21,000 have been delivered, they have had twins born once every ſixty-ſecond time. The average of which is once in 78 births nearly, in theſe kingdoms.

The calculations made in Germany from great numbers, in various ſituations, ſtate twins as happening in a varied proportion from once every ſixty-fifth to once every ſeventieth time.

But in a more accurate and later calculation made at Paris, by M. TENON, Surgeon to the Salpêtriere, we learn, that in 104,591 births the proportion of twins was only one in 96, which is only a ſmall degree leſs than we have calculated at the Britiſh Lying-in Hoſpital.

It would be eaſy to add other calculations, all differing from theſe and from one another, more or leſs; but I hope theſe are ſufficient to ſhew that nature obſerves no certain rule in this matter;

matter; and that even twins, the most usual variation, is not a very common occurrence.

When we advance to triplets, or three born at once, we find comparatively very few instances in this or any other country; and though every one has heard of such events as now and then happening, yet very few have seen them.

In all those 18,300 women delivered at the British Lying-in Hospital, there has not been one such case. In the London Lying-in Hospital, where, being instituted later, much fewer have been delivered, they have had two such recorded as prodigies. In the Westminster Dispensary, in 1897 women delivered, there has been but one such event.

In the Dublin Hospital, in 21,000 births, they have had triplets born thrice, or once in 7000 times, but have never exceeded that proportion or number, born at one time.

In a pretty extensive practice of above thirty years, both in the county of Rutland and in London, I have attended but one labour where three children were born; am personally acquainted but with one lady who, at Dumfries, in Scotland, after bearing twins twice, was delivered of three children at once; and I was never acquainted with any one who produced a greater number.

Yet so much does this matter vary at Edinburgh, that Dr. HAMILTON, Professor of Midwifry, writes, he had seen triplets born there, five or six times in less than twenty-five years.

MAURICEAU, in a long life of very extensive practice at Paris, with opportunities of knowing most things extraordinary that happened in his time in France, tells us, he had seen triplets born but a few times; had heard of four in that city but once, and mentions no greater number.

One circumstance which he relates is so far worthy of attention, as it accords with one somewhat similar subjoined to Mr.

HULL's caſe now read, *viz.* "That the huſband of one of "thoſe women who bore three children was by trade a pain- "ter, and had been, for two years preceding this birth, para- "lytic over one-half of his body, and yet had no reaſon to "doubt the fidelity of his wife."

Theſe facts, as far as they are to be depended on, may ſhew us, that the capacity of procreation in the male may remain under very infirm health; and that we ought to judge with candour of ſuch wives as are fruitful when living with very ailing huſbands, and who produce healthy children in the eighth, or even ninth, month after their death; as we can never ſay determinately under what degree of diſeaſe the male is totally incapable of procreation: more eſpecially as we are very certain, that the female is not, when labouring under very deſperate, and certainly fatal, diſeaſes, provided the principal organs of generation be ſound. Nay, in caſes of pulmonary phthiſis, the life of the female ſeems to be protracted by pregnancy; and I have attended a lady, who, after being pronounced irrecoverably hectic, lived long enough to be twice delivered naturally of healthy children at the full time.

But what particular circumſtances of conſtitution, or ſtate of health, can capacitate the male to become the father of more than one child at a birth, or how this could be effected, ſhould it be wiſhed, remains among thoſe ſecrets of nature which our want of facts and obſervations renders us utterly incapable to ſpeculate upon.

It ſeems probable, and theſe two obſervations, as well as SPALLANZANI's, and other late experiments, would rather incline us to ſuppoſe, that theſe numerous births do depend moſt on the ſtructure and ſtate of the female organs; but

nothing, that I know of, has ever been discovered in this obscure matter.

The occurrence of four born at once we find to be much more uncommon; and, I think, HALLER's conjecture rather than calculation of its happening once in 20,000 births, very much under-rated, as it appears that once in 100,000 would be much nearer the truth. Of this, however, we have several well authenticated cases which have happened in this island. In the year 1674, there was published in London a quarto pamphlet, intituled, "The fruitful Wonder, or a strange Re-"lation, from Kingston upon Thames, of a Woman who, "on Thursday and Friday, the Fifth and Sixth Days of this "Instant March, 1673-4, was delivered of Four Children at "one Birth, *viz.* Three Sons and One Daughter, all born "alive, lusty Children, and perfect in every Part, which lived "Twenty-four Hours, and then died, all much about the "same Time, with several other Examples of numerous "Births, from credible Historians, with the Physical and Astro-"logical Reasons for the same. By J. P. Student in Physic."

Dr. PLOTT, in his History of Staffordshire, p. 194. mentions ELEANOR, the wife of HENRY DIVEN, of Watlington, who was delivered of four children at a birth in the year 1675.

Sir ROBERT SIBBALD, in his *Scotia Illustrata*, after mentioning a case of three born at once, adds, "Imo in variis "regni locis repertæ sunt mulieres quæ quatuor fœtus uno "partu ediderunt;" but makes no mention of more.

In the Gentleman's Magazine, which is reckoned a pretty authentic record of the times, we have the following accounts of numerous births.

ANN BOYNTON, of Henſbridge, in Somerſetſhire, was this day, June 1, 1736, delivered of three daughters and one ſon; one of the daughters died, the reſt are likely to live. The mother has been married but four years, and has had twice twins before, which completes the number of eight children at three births.

October 3, 1743, at Rate, in Berkſhire, JOAN GALLOWAY was delivered of two boys and two girls, three of whom were alive.

In January, 1746, the wife of PLUMER, a labouring man, at Mill-Wimley, near Hitchin, Hertfordſhire, was delivered of three living boys and one dead.

Auguſt 22, 1746, the wife of WILLIAMS, of Coventry-ſtreet, Piccadilly, was delivered of two boys and two girls, all likely to live.

June, 1752, a woman in the pariſh of Tillicultrie, near Stirling, in Scotland, was delivered of four children, which were all immediately baptiſed, and all died at the ſame time next morning.

In September, 1757, a poor woman, of Burton Ferry, Glamorganſhire, was delivered of three boys and a girl.

Dr. HAMILTON before mentioned writes, that, not many years ago, a woman was delivered of four children, at Pennycuick, the ſeat of Sir JOHN CLARK, Bart. near Edinburgh, when ſhe was advanced to the middle of her laſt month of pregnancy, and that ſome of theſe children lived two or three years. He further ſays, that, five years ago, he attended a woman at Edinburgh, who, in the ſeventh month of her pregnancy, after a journey of thirty miles, was ſuddenly delivered of four children, all perfect and well grown for the time, of which one was born dead, and three alive; but thoſe three

died next day. He further adds, that theſe are the only caſes of quadruplets, or any larger number, he had ever heard of, as born in Scotland, in his memory.

Though caſes ſimilar to the preſent of five children born at once, are ſtill much more uncommon; and though HALLER's aſſertion of their not happening above once in a million of births, may be reckoned a very moderate calculation, yet we are not altogether without ſuch inſtances in this country.

From the Gentleman's Magazine we learn, that on the 5th of October, 1736, a woman at a milk-cellar, in the Strand, was delivered of three boys and two girls at one birth; and that in March, 1739, at Wells, in Somerſetſhire, a woman was delivered of four ſons and a daughter, all alive, all chriſtened, and all then ſeeming likely to live.

In the *Commercium Literarium Norimbergenſe* for the year 1731, we have two ſuch caſes; one happening in Upper Saxony, the other near Prague, in Bohemia; in each of which five children were born and chriſtened, all of whom were arrived to that equal degree of maturity, which rendered it probable, they were all conceived about the ſame time.

I learned from two foreign Profeſſors, when in London laſt winter, that they had each heard of a caſe of five children born near Paris, and near Ghent in Flanders; but the particulars not being ſent as promiſed, I preſume they may have been miſinformed.

When we advance farther we get into the region of tradition and improbability; and it would ill become me to trouble a Society, whoſe profeſſed object is truth and ſcience, with the numerous and wonderful relations which many grave and learned authors have recorded as facts they themſelves believed; yet I ſtill think we have no authority to reject abſolutely every relation

relation of this kind, when AMBROSE PAREY, a very honeſt though credulous man, tells, that in his time, in the pariſh of Sceaux, near Chambellay between Sarte and Maine, the mother of the then living lord of the noble houſe of MALDEMEURE had, in the firſt year of her marriage, brought forth twins, in the ſecond triplets, in the third four, in the fourth five, and in the fifth year ſix children at one birth, of which labour ſhe died; and when he adds, that of theſe *laſt ſix* one is yet alive, and is now Lord of Maldemeure, how can we diſbelieve this circumſtance? This ſtory may very poſſibly be inaccurately ſtated, yet the whole cannot be a fiction, as it was publiſhed among the very people, and in the age when it happened, and never has been ſince contradicted ſo far as we know. Though the wonderful regularity of the progreſs gives an appearance of fable to the whole, yet we muſt believe the thing to be poſſible: and that this then exiſting lord might be the only one of the ſix who lived long enough to be born at the full time, in a mature ſtate; the whole, or moſt of the other five, as we have ſometimes ſeen in caſes of twins, having been born as dead abortions, which had never arrived to a bulk ſufficient to interfere with his growth.

I leave the learned to pay what degree of credit they pleaſe to the wonderful relations we read of the extreme fertility of the women of Egypt, Arabia, and other warm countries, as recorded by ARISTOTLE, by PLINY, and by ALBUCASIS, where three, four, five, and ſix children are ſaid to have been frequently born at once, and the greateſt part of theſe reared to maturity; and will only ſay, that though a late traveller M. SAVARY gives ample teſtimony of the extreme general fertility of Egypt in all vegetable and animal productions, and particularly

ricularly of its abundant population, he mentions nothing of the numerous births recorded by the ancient naturalists and historians.

Of still more fruitful births I will pass over a number of instances which I could adduce from JOHANNES RHODIUS, LUCAS SCHROECKIUS, CASPAR BAUHIN, JOHANNES HELVIGIUS, BIANCHI, and others, and finish with one case more, recorded by PETRUS BORELLI in his Second Century of Observations, published at Paris in the year 1656; a collection indeed filled with many wonderful stories, though by a man of equal integrity and ingenuity: he tells us, that in the year 1650, just five years before, the lady of the then present Lord DARRE produced at one birth eight perfect children, which he owns was a very unusual event in that country.

I think it totally unnecessary to pursue this enquiry farther; but must observe, that the present is the only case I have found, where the children were all females; that the males have in all the other cases been at least equal, and generally the most numerous; that in many of them, at least a part was dead born; and that most commonly the rest died in a short time. It is thence clear, that those numerous births are certainly unfavourable to population, as very few indeed of those children can be carried to near the full term of pregnancy, and fewer still to that degree of strength that admits of their being reared, where more than two are born at one time.

As from Mr. JOHN HUNTER's very curious Experiments and Observations, read lately to this Society, on the Procreation of Swine, we are led to believe, that a certain determined number of ova, capable of receiving male impregnation, are originally formed in each ovarium; and which number, when exhausted, the female constitution has no power to renew; if this be the

true

true account of the oeconomy of nature in this particular, which has every appearance of probability, thoſe numerous births muſt occaſion a very fruitleſs profuſion and waſte of the human race, and become every way detrimental to its increaſe.

From the united teſtimony of all the foregoing caſes, it is undeniably clear, that the females of the human ſpecies, though moſt commonly uniparous, are, in certain circumſtances to us unknown, every now and then capable of very far exceeding their uſual number; and I muſt again repeat, that it does not appear that we can ſet any bounds to the powers of nature in that reſpect; or pretend, as ſome have done, with certainty to ſay, what may be the utmoſt limits of human fertility.

XXXV. Chloranthus, *a new Genus of Plants, described by* Olof Swartz, *M. D. Communicated by Sir* Joseph Banks, *Bart. P. R. S.*

Read June 21, 1787.

AMONG the numberleſs vegetable productions that have appeared in the Royal Garden at Kew, is the preſent. It is already long ſince this curious plant has been introduced there as a native of China, where, we are told, the ſame is cultivated in the Chineſe gardens, though it ſeems not to have any qualities either palatable or odoriferous, nor a beautiful appearance.

At the firſt ſight of the plant, there is ſome likeneſs of Viſcum or Loranthus; and conſidering the inflorescentia, and the inſertion of the antheræ, we find no leſs analogy; though on a nearer examination it is greatly different, and of a very intricate conſtruction.

The pains I have taken to enucleate the family relation of this hitherto unknown vegetable, have induced me, for the ſake of its ſingularity, to preſent it as a new genus, of which I think the following natural character may be the moſt proper:

Calyx nullus; ſed

Squama ovata, acuta, concava, cui germen inſidens.

Corolla monopetala, dimidiata, vel

Petalum unicum, ſubrotundum, trilobum, convexum, lateri exteriori germinis inſertum, ſtaminiferum, deciduum.

Lobus intermedius cæteris major.

Stam. Filamenta nulla.

Antheræ quatuor, marginibus petali longitudinaliter accretæ, bivalves.

Piſt. Germen oblongum, vel obovatum difforme, ſquama fere tectum, antice prominens, petaligerum.

Stylus obliquus, craſſus, breviſſimus, angulatus.

Stigmata tria, minutiſſima, erecta.

Per. Bacca oblonga, monoſperma.

Semen oblongum.

From this generical deſcription the eſſential character is formed:

Calyx nullus.

Cor. Petalum trilobum lateri germinis inſidens.

Antheræ petalo accretæ.

Bacca monoſperma.

This new genus is to be placed in Tetrandria Monogynia with *flores incompleti*, *ſuperi*, next after *Acæna*; and amongſt the *Ordines naturales* I think it would beſt take its place in the XLVIII. ſ. next after *Viſcum*.

To diſtinguiſh this ſpecies from others, that may be diſcovered of the ſame genus, I have adopted the *nomen triviale* of

Chloranthus inconſpicuus:

Its ſpecific deſcription is as follows:

Planta herbacea.

Caules plures ex radice, ſemipedales, patentes, ſuberecti, ramoſiuſculi, teretes, glabri.

Rami oppoſiti, patentes, teretes, ſtriati, glabri.

Folia petiolata, decuſſata, oppoſita, lanceolato-ovata, margine ſerrata, nervoſa, venoſa, ſubſucculenta, glaberrima, pallide viridia.

Petioli

Petioli breviuſculi, ſuperne canaliculati, glabri.

Stipulæ interpetiolares, utrinque denticulis duobus minutis, membranaceæ, perſiſtentes.

Flores paniculati.

Panicula terminalis, erecta, ſimplex.

Racemi (vel *ſpicæ*) oppoſiti, decuſſati, erectiuſculi, ſubfaſtigiati.

Flores oppoſiti, decuſſati, ſeſſiles, ſolitarii, minuti, magnitudine capitis aciculæ, ſubſucculenti, ex albido lutei.

Pollen flavum.

Stigmata albida.

Bacca nigra, magnitudine piperis.

I have not been able to find any deſcription or figure anſwering to this plant in the works of the Eaſt-Indian naturaliſts. I have only met with one Chineſe drawing, in the library of Sir Joseph Banks, Bart. P. R. S. among ſome others of their garden plants, that ſeems to repreſent the preſent.

It is ſaid to be called Chu-Lan by the Chineſe; but it ought not to be confounded with the Tsjiulang or Camunium Chinenſe of Rumphius (Herb. Amboin. l. VII. cap. xv. and Auctuarii ejuſd. cap. xlvii.), the deſcription of which ſeems to correſpond in ſome parts with the Chloranthus: the *firſt* figure, however, on the eighteenth plate ſhews the plant of Rumphius to be the Vitex pinnata of Linnæus.

EXPLANATION OF THE PLATE. (Tab. XIV.)

The plant in natural size.

a. A part of a racemus with flowers, magnified.

b. A flower magnified, separated from the racemus, containing the pistillum, with the petalum inserted on the side of the germen.

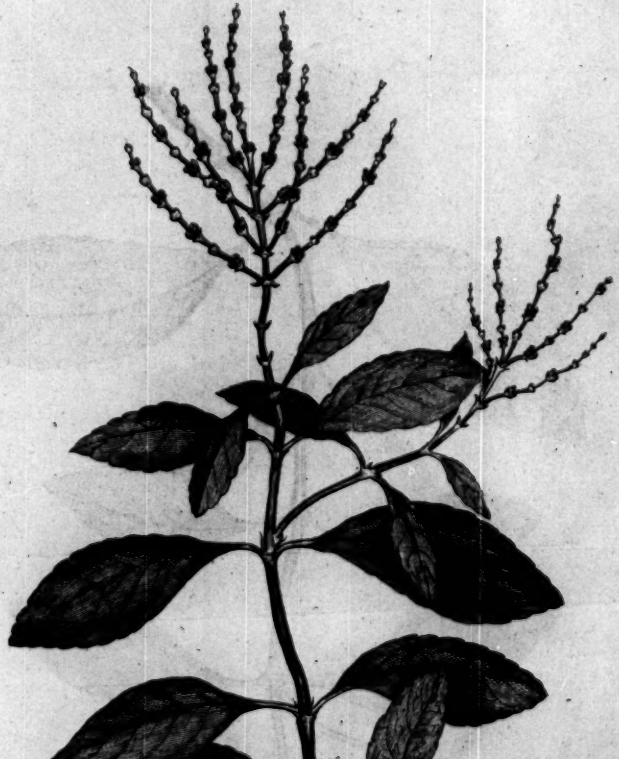
c. A petalum separately, with its four antheræ inserted on the margins of the lobes.

d. The germen without the corolla.

e. The germen when in flower, cut transversely, shewing the rudiment of the seed.

f. The bacca, cut transversely, with one room and seed, of natural size.

g. The form of the seed.



Chloranthus inconspicuus.

A. Swartz del.

Bajire Sc.

XXXVI. *On the Precession of the Equinoxes.* By the Rev. Samuel Vince, *M. A. F. R. S.*

Read June 21, 1787.

1. THE true cause of the precession of the equinoctial points was first assigned by Sir ISAAC NEWTON; but it is confessed, that he has fallen into an error in his investigation of the effect. Without, however, entering into any enquiry relative to the circumstances in which he has erred, I propose to shew how we may obtain a true solution from his own principles, by means of which alone the whole calculation may be rendered extremely simple and evident: and although very satisfactory solutions have been already given, yet the importance of the problem will sufficiently apologise for offering any thing further upon the subject that may at all tend to elucidate it.

2. Let S (Tab. XV. fig. 1.), be the sun, ABDC the earth, T its center, EQ the equator, P, p, the poles; draw CTB perpendicular to SAD, and join SE, which produce to meet CB in K. Call the radius ET unity, and let the force of the sun on a particle at T be $\frac{1}{ST^2}$, then the force on a particle at E $= \frac{1}{SE^2}$; hence, if we resolve this latter force into two others, one in the direction ET, and the other in the direction parallel to TS, we have SE : ST :: $\frac{1}{SE^2}$: the force in the direction parallel to

to $TS = \frac{ST}{SE^3} = \frac{ST}{ST - EK^3} = \frac{1}{ST^2} + \frac{3EK}{ST^3}$, omitting the other terms of the ſeries on account of their ſmallneſs. Hence the force with which a particle at E is drawn *from* CB is equal to $\frac{3EK}{ST^3}$; conſequently the effect of this force in a direction perpendicular to ET will be $\frac{3EK \times KT}{ST^3}$; hence this force : the force of the ſun on a particle at T :: $\frac{3EK \times KT}{ST^3} : \frac{1}{ST^2}$ :: 3EK × KT : ST. Now if P = the periodic time of the earth, p = the periodic time of a body revolving at the earth's ſurface; then the force of the earth to the ſun : the force of the body to the earth, or the force of gravity, :: $\frac{ST}{P^2} : \frac{1}{p^2}$; and hence the force of the ſun on a particle at E perpendicular to ET : the force of gravity :: $\frac{3EK \times KT \times p^2}{P^2} : 1$.

3. Let v be the center of gyration, and put M = the quantity of matter in the earth : then the effect of the inertia of M placed at v, to oppoſe the communication of motion, is the ſame as the effect of the inertia of the earth; and hence, by the property of that center, $ET^2 : Tv^2 \left(= \frac{2}{5} ET^2\right) :: M : \frac{2}{5} M$, which is the quantity of matter to be placed at E to have the ſame effect.

4. Put m = the exceſs of the quantity of matter in the earth above that of its inſcribed ſphere. Now by Sir ISAAC NEWTON's two firſt lemmas, it appears, that the action of the ſun upon the ſhell m of matter, to generate an angular velocity about an axis perpendicular to CABD, is juſt the ſame as it would be to generate an angular velocity in a quantity of matter equal to $\frac{1}{5} m$ placed at E. Let us therefore ſuppoſe the ſun's attraction, perpendicular to ET, to be exerted upon a

quantity of matter at E equal to $\frac{1}{2}m$, and at the same time to have a quantity of matter to move equal to $\frac{1}{2}$M, and then from this and art. 3. it appears, that the effect will be the same as the accelerative force of the sun to turn about the earth. Hence that accelerative force is, from art. 2. equal to $\frac{3EK \times KT \times p^2 \times \frac{1}{2}m}{\frac{1}{2}M \times P^2} = \frac{3EK \times KT \times p^2 \times m}{2M \times P^2}$, gravity being unity. Now, if TE : TP :: 1 : $1-r$, then M : M$-m$ :: 1 : $1-2r$, therefore, M : m :: 1 : $2r$, hence $\frac{m}{2M} = r$, consequently the accelerative force $= \frac{3EK \times KT \times p^2 \times r}{P^2}$.

5. Let $\dot{x}$ = the arc described by a point of the equator about its axis in an indefinitely small given time, which may therefore represent its velocity; and let $a\dot{x}$ represent the arc described in the same time by a body revolving about the earth at its surface; then $\frac{a^2\dot{x}^2}{2}$ = the sagitta of the arc described by the body in the same time, and consequently $a^2\dot{x}^2$ = the velocity generated by gravity whilst a point of the equator describes $\dot{x}$. Hence, by art. 4. we have 1 : $\frac{3EK \times KT \times p^2 \times r}{P^2}$ :: $a^2\dot{x}^2$: $\frac{3EK \times KT \times p^2 \times r \times a^2\dot{x}^2}{P^2}$ the velocity of the point E of the equator generated by the action of the sun, whilst the equator describes $\dot{x}$ about its axis; consequently the ratio of these velocities is as $\frac{3EK \times KT \times p^2 \times a^2\dot{x}}{P^2}$: 1.

6. Let $\dot{y}$ be an arc described by the sun in the ecliptic to a radius equal to unity, whilst a point of the equator describes $\dot{x}$ about its axis; then (as ap = the time of the earth's rotation, and the arcs described in equal times to equal radii are inversely as the periodic times) $\frac{1}{P} : \frac{1}{ap} :: \dot{y} : \dot{x} = \frac{P\dot{y}}{ap}$; hence, if v and w be put

for

for the sine and cosine of the sun's declination, the ratio of the velocities in the last article becomes $\frac{3apvvwy}{P} : 1$.

7. Hence if TSL (fig. 2.) be the ecliptic to the radius unity, P the plane of the sun, SER the equator, PE the sun's declination, and we take $Ec : cd$ (cd being perpendicular to Ec) $:: 1 : \frac{3aprvwy}{P}$, and through d, E, describe the great circle TEM, then will ST be the precession of the equinox during the time the sun describes y in the ecliptic. Now Ed (or Ec, as the angle at E is indefinitely small) : dc :: rad. $= 1$: sine angle $E = \frac{3aprvwy}{P}$; hence (if SV be drawn perpendicular to TE) 1 : sine SE :: $\frac{3aprvwy}{P}$: $SV = \frac{3aprvw \times \text{sin. SE} \times y}{P}$; therefore, sin. STV or ESP : 1 :: SV : $ST = \frac{3aprvw \times \text{sin. SE} \times y}{P \times \text{sin. ESP}}$.

8. Now $\frac{v}{\text{sin. ESP}} = \text{sin. SP}$, and $vw = \frac{\text{cos. SP}}{\text{cos. ES}}$, hence $\frac{vw}{\text{sin. ESP}} = \frac{\text{sin. SP} \times \text{cos. SP}}{\text{cos. ES}}$; but $\frac{\text{cos. ESP}}{\text{tan. ES} \times \text{cot. SP}} = 1$, hence $\frac{vw}{\text{sin. ESP}} = \frac{\text{sin. SP} \times \text{cos. SP} \times \text{cos. ESP}}{\text{cos. ES} \times \text{tan. ES} \times \text{cot. SP}} = \frac{\text{sin. SP}^2 \times \text{cos. ESP}}{\text{sin. ES}}$; consequently,

$ST = \frac{3apr \times \text{sin. SP}^2 \times \text{cos. ESP} \times y}{P} =$ (if $x = \text{sin. SP}$) $\frac{3apr \times \text{cos. ESP} \times x^2 \dot{x}}{P \times \sqrt{1 - x^2}}$, whose fluent, when $x = 1$, is $\frac{3apr \times \text{cos. ESP} \times y}{2P}$ (y being now = to a quadrant) the arc of precession whilst the sun describes 90° of the ecliptic; and to find the degrees say, $4y : 360° :: \frac{3apr \times \text{cos ESP} \times y}{2P} : 360° \times \frac{3apr \times \text{cos. ESP}}{8P}$, consequently the precession in a year $= 360° \times \frac{3apr \times \text{cos. ESP}}{2P} = 21'' \; 6'''$. This would be the precession of the equinox arising from the attraction of the sun, if the earth were of an uniform density, and the ratio of

Fig. 1.

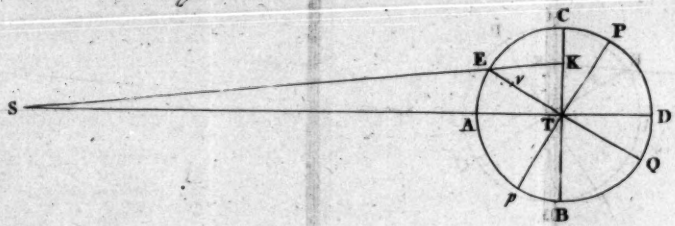
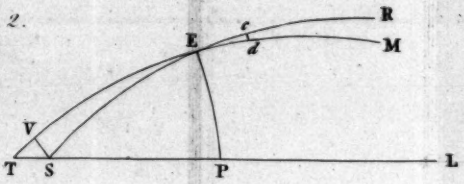


Fig. 2.



of the diameters as 229 : 230; but if the greateſt nutation of the earth's axis be rightly aſcertained, the preceſſion is only about $14''\frac{1}{2}$; which difference between the theory and what is deduced from obſervation muſt ariſe either from the fluidity of the earth's ſurface, an increaſe of denſity towards the center, or the ratio of the diameters being different from what is here aſſumed, or probably from all the cauſes conjointly. But as the beſt obſervations muſt be liable to ſome ſmall degree of inaccuracy, and an error of one or two ſeconds in the nutation will, in this caſe, make a very conſiderable alteration in the concluſion, the eſtimation of the preceſſion ariſing from the action of the ſun ſeems to be ſubject to a very conſiderable degree of uncertainty.

XXXVII. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain at* Lyndon *in* Rutland, *in* 1786. *By* Thomas Barker, *Eſq. Alſo of the Rain at* South-Lambeth, *in* Surrey; *and at* Selbourn *and* Fyfield, Hampſhire. *Communicated by* Thomas White, *Eſq. F.R.S.*

Read June 21, 1786.

		Barometer.			Thermometer.						Rain.			
					In the Houſe.			Abroad.						
		Higheſt	Loweſt	Mean.	High.	Low.	Mean	High	Low.	Mean	Lyndon	S. Lambeth.	Selbourn.	Fyfield.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	29,87	28,33	29,18	49	25	38	48¼	11½	34	3,467	2,48	6,58	4,93
	Aftern.				50	25	39	53	19	39				
Feb.	Morn	29,96	28,84	29,50	48	32½	40	45½	24	34	0,665	1,08	1,27	4,78
	Aftern				46½	33	41	49	27½	39½				
Mar.	Morn.	29,78	28,76	29,29	46	28	37	46	18	30	0,832	1,11	1,53	1,64
	Aftern.				47½	29	38	50½	22½	39				
Apr.	Morn.	29,96	29,02	29,46	55½	38½	47	52	29	41	1,252	1,22	1,63	1,41
	Aftern.				57	40	48	68	37	51				
May	Morn.	29,90	28,75	29,46	63½	45	53	59½	32	48½	2,383	0,97	2,16	2,79
	Aftern.				65½	46½	55	73	49	59½				
June	Morn.	29,99	29,32	29,53	66	58½	62	65	49	56½	1,583	2,24	1,05	1,51
	Aftern.				69	59	64	80½	60	68				
July	Morn.	30,00	29,05	29,56	65	56½	61½	62½	50	56	1,799	0,86	1,81	1,42
	Aftern.				66½	58½	63	74	51	66				
Aug.	Morn.	29,84	20,01	29,43	67	57	61	65	48	55	2,632	1,19	4,00	3,57
	Aftern.				68	58½	62	75½	57	65½				
Sept.	Morn.	29,98	28,40	29,35	61	49	55	58	38	47	2,840		4,50	1,62
	Aftern.				61	49	56½	67	47	57				
Oct.	Morn.	30,00	28,29	29,55	54½	45	49	50	31	41	4,762	8,22	5,04	4,18
	Aftern.				55½	45½	50	62½	42	49				
Nov.	Morn.	29,81	28,55	29,36	45	36½	41	45	27	35	2,938		4,38	1,22
	Aftern.				45½	36½	41½	47½	30	39				
Dec.	Morn.	30,05	28,44	29,15	46	32	39½	45½	Thermom.		2,136	3,06	5,62	4,13
	Aftern.				46	33	40½	46	broken.					
										Inches	27,289	22,43	39,57	29,60

The

The froſts at the end of 1785 and beginning of 1786 were ſevere, but not long, with large ſnows the end of December and middle of January. The intervals were wet in January and windy, but mild in February. At this time ſome farmers ſowed a little barley, which, after lying a great while in the ground during the following froſt, came up well at laſt, was forward, and proſpered. The end of February and beginning of March was the longeſt froſt this winter, being a full fortnight, and the wind being ſtrong from the eaſt, and no ſnow at firſt, and when things were getting forward, it did more hurt than all the froſts this winter; and the winds continued much N.E. and frequently froſty the reſt of the month. The ſeed time was good, but rather backward, and the weather in general dry in February, March, and April, the wind often N.E. and the ſeaſon backward, yet not ſo many froſty nights as in ſome late ſprings, and the latter half of April mild and growing. After ſome froſty mornings the beginning of May it was in general a growing month, and a fine rain before the middle made plenty of graſs; and the latter end of May, and moſt part of June and July, being fine and moderately hot, I think, I never knew ſo much hay ſo well got in any year before, which was of great ſervice, as there was ſcarce any old left. The firſt ſown turneps ſtood very well; but the dryneſs of the ſeaſon hindered the latter ſown from coming up well till Auguſt, and many were ſmall; and the ſeaſon was drier, and the ground more burnt, in the north of England than here.

The laſt day of July began a cool, ſhowery ſeaſon, which much improved the graſs and turneps, but hindered the beginning of harveſt, which, however, was afterward well gotten. The crop of barley this year was great, the wheat good, oats indifferent: but a great part of the beans never came up well;

whether becauſe the ſeed was ill gotten laſt year, or becauſe a froſty March after they were ſown ſpoiled them. I think I ſcarce ever knew more north winds in ſummer, or more eaſt winds in October or November, than this year. From Auguſt to December there was a great deal of rain by fits, particularly the middle of Auguſt, about Michaelmas, the 6th to the 12th of October, the third week in November, and firſt half of December; yet with intervals of fair fine weather between. It was much windy, and ſome great ſtorms on September 14, October 8, and December 14. The N.E. winds in October and November brought on cold weather early; and the accounts from the northern countries complained of a ſevere beginning of winter, almoſt ſhutting up the Baltic before the uſual time; but the reſt of the winter proved very different. After the rains in the former part of December, the year ended froſty, and to Chriſtmas as ſharp as any this winter; but no way remarkable, and with little of either rain or ſnow.

Since I ſaw that corona about the Moon, mentioned in Philoſophical Tranſactions, Vol. LXXIII. p. 245. I have ſome few times ſeen a very faint appearance of it, and made the following remarks about it. The common bright circle round the moon, bounded by a yellowiſh red, is of much larger diameter, more diffuſed, and fainter, when the air is warm and the clouds miſty; but no corona then appears. The time to expect it is in a froſt, or when inclined to it, the clouds better defined and white; that firſt circle is then much leſs in diameter and brighter. It was remarkably ſo when the corona appeared November 17, 1782; and it has always been ſo in ſome meaſure, whenever there has been any tendency to it ſince.

XXXVIII. *Obſervations on the Structure and Oeconomy of* Whales. *By* John Hunter, *Eſq. F. R. S.*; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read June 28, 1787.

THE animals which inhabit the ſea are much leſs known to us than thoſe found upon land; and the œconomy of thoſe with which we are beſt acquainted is much leſs underſtood: we are, therefore, too often obliged to reaſon from analogy where information fails; which muſt probably ever continue to be the caſe, from our unfitneſs to purſue our reſearches in the unfathomable waters.

This unfitneſs does not ariſe from that part of our œconomy on which life and its functions depend; for the tribe of animals which is to be the ſubject of this Paper, has, in that reſpect, the ſame œconomy as man, but from a difference in the mechaniſm by which our progreſſive motion is produced.

The anatomy of the larger marine animals, when they are procured in a proper ſtate, can be as well aſcertained as that of any others; dead ſtructure being readily inveſtigated. But even ſuch opportunities too ſeldom occur, becauſe thoſe animals are only to be found in diſtant ſeas, which no one explores in purſuit of natural hiſtory; neither can they be brought to us alive from thence, which prevents our receiving their bodies in a

ſtate

ſtate fit for diſſection. As they cannot live in air, we are unable to procure them alive.

Some of theſe aquatic animals yielding ſubſtances which have become articles of traffic, and in quantity ſufficient to render them valuable as objects of profit, are ſought after for that purpoſe; but gain being the primary view, the reſearches of the Naturaliſt are only conſidered as ſecondary points, if conſidered at all. At the beſt, our opportunities of examining ſuch animals do not often occur till the parts are in ſuch a ſtate as to defeat the purpoſes of accurate enquiry, and even theſe occaſions are ſo rare as to prevent our being able to ſupply, by a ſecond diſſection, what was deficient in a firſt. The parts of ſuch animals being formed on ſo large a ſcale, is another cauſe which prevents any great degree of accuracy in their examination; more eſpecially when it is conſidered, how very inconvenient for accurate diſſections are barges, open fields, and ſuch places as are fit to receive animals, or parts, of ſuch vaſt bulk.

As the opportunities of aſcertaining the anatomical ſtructure of large marine animals are generally accidental, I have availed myſelf, as much as poſſible, of all that have occurred; and, anxious to get more extenſive information, engaged a Surgeon, at a conſiderable expence, to make a voyage to Greenland, in one of the ſhips employed in the whale fiſhery, and furniſhed him with ſuch neceſſaries as I thought might be requiſite for examining and preſerving the more intereſting parts, and with inſtructions for making general obſervations; but the only return I received for this expence was a piece of whale's ſkin, with ſome ſmall animals ſticking upon it. From the opportunities which I have had of examining different animals of this order, I have gained a tolerably accurate idea of the

anatomical ſtructure of ſome genera, and ſuch a knowledge of the ſtructure of particular parts of ſome others, as to enable me to aſcertain the principles of their œconomy.

Thoſe which I have had opportunities of examining were the following:

Of the Delphinus Phocæna, or Porpoiſe, I have had ſeveral, both male and female.

Of the Grampus I have had two; one of them (Tab. XVI.) twenty-four feet long, the belly of a white colour, which terminated at once, the ſides and back being black; the other (Tab. XVII.) about eighteen feet long, the belly white, but leſs ſo than in the former, and ſhaded off into the dark colour of the back.

Of the Delphinus Delphis, or Bottle-noſe Whale (Tab. XVIII.) I had one ſent to me by Mr. JENNER, Surgeon, at Berkeley. It was about eleven feet long. I have alſo had one twenty-one feet long, reſembling this laſt in the ſhape of the head, but of a different genus, having only two teeth in the lower jaw (Tab. XIX.); the belly was white, ſhaded off into the dark colour of the back. This ſpecies is deſcribed by DALE, in his Antiquities of Harwich. The one which I examined muſt have been young; for I have a ſkull of the ſame kind, nearly three times as large, which muſt have belonged to an animal thirty or forty feet long.

Of the Balæna roſtrata of FABRICIUS, I had one, ſeventeen feet long (Tab. XX.).

The Balæna Myſticetus, or large Whalebone Whale, the Phyſeter Macrocephalus, or Spermaceti Whale, and the Monodon Monoceros, or Narwhale, have alſo fallen under my inſpection. Some of theſe I have had opportunities of examining with accuracy; while others I have only examined in part,

part, the animals having been too long kept before I procured them, to admit of more than a very ſuperficial inſpection.

From theſe circumſtances it will be readily ſuppoſed, that an accurate deſcription of all the different ſpecies is not to be expected; but having acquired a general knowledge of the whole tribe, from the different ſpecies which have come under my examination, I have been enabled to form a tolerable idea, even of parts which I have only had the opportunity of ſeeing in a very curſory way.

General obſervation would lead us to believe, that the whole of this tribe conſtitutes one order of animals, which Naturaliſts have ſubdivided into genera and ſpecies; but a deficiency in the knowledge of their œconomy has prevented them from making theſe diviſions with ſufficient accuracy; and this is not ſurpriſing, ſince the genera and ſpecies are ſtill in ſome meaſure undetermined even in animals with which we are better acquainted.

The animals of this order are in ſize the largeſt known, and probably, therefore, the feweſt in number of all that live in water. Size, I believe, in thoſe animals who feed upon others, is in an inverſe proportion to the number of the ſmaller; but, I believe, this tribe varies more in that reſpect than any we know, viewing it from the Whalebone Whale, which is ſeventy or eighty feet long, to the Porpoiſe that is five or ſix: however, if they differ as much among themſelves as the Salmon does from the Sprat, there is not that comparative difference in ſize that would at firſt appear. The Whalebone Whale is, I believe, the largeſt; the Spermaceti Whale the next in ſize (the one which I examined, although not full grown, was about ſixty feet long); the Grampus, which is an extenſive genus, is probably

probably from twenty to fifty feet long; under this denomination there is a number of ſpecies.

From my want of knowledge of the different genera of this tribe of animals, an incorrectneſs in the application of the anatomical account to the proper genus may be the conſequence; for when they are of a certain ſize, they are brought to us as Porpoiſes; when larger, they are called Grampus, or Fin-fiſh. A tolerably correct anatomical deſcription of each ſpecies, with an accurate Drawing of the external form, would lead us to a knowledge of the different genera, and the ſpecies in each; and, in order to forward ſo uſeful a work, I propoſe, at ſome future period, to lay before the Society deſcriptions and drawings of thoſe which have come under my own obſervation.

From ſome circumſtances in their digeſtive organs we ſhould be led to ſuppoſe, that they were nearly allied to each other; and that there was not the ſame variety, in this reſpect, as in land animals.

In the deſcription of this order of animals, I ſhall always keep in view their analogy to land animals, and to ſuch as occaſionally inhabit the water, as white Bears, Seals, Manatees, &c. with the differences that occur. This mode of referring them to other animals, better known, will aſſiſt the mind in underſtanding the preſent ſubject. It is not, however, intended in this Paper to give a particular account of the ſtructure of all the animals of this order, which I have had an opportunity of examining: I propoſe, at preſent, chiefly to confine myſelf to general principles, giving the great outlines as far as I am acquainted with them, minuteneſs being only neceſſary in the inveſtigation of particular parts.

In my account I ſhall pay ſome attention to the relations of men who have given facts without knowing their cauſes, whenever I find that ſuch facts can be explained upon true principles of the animal œconomy, but no further.

This order of animals has nothing peculiar to fiſh, except living in the ſame element, and being endowed with the ſame powers of progreſſive motion as thoſe fiſh that are intended to move with a conſiderable velocity: for I believe, that all that come to the ſurface of the water (which this order of animals muſt do) have conſiderable progreſſive motion; and this reaſoning we may apply to birds; for thoſe which ſoar very high have the greateſt progreſſive motion.

Although inhabitants of the waters, they belong to the ſame claſs as quadrupeds, breathing air, being furniſhed with lungs, and all the other parts peculiar to the œconomy of that claſs, and having warm blood; for we may make this general remark, that in the different claſſes of animals there is never any mixture of thoſe parts which are eſſential to life, nor in their different modes of ſenſation.

I ſhall divide what is called the œconomy of an animal, firſt, into thoſe parts and actions which reſpect its internal functions, and on which life immediately depends, as growth, waſte, repair, ſhifting or changing of parts, &c. the organs of reſpiration and ſecretion, in which we may include the powers of propagating the ſpecies.

Secondly, into thoſe parts and actions which reſpect external objects, and which are variouſly conſtructed, according to the kind of matter with which they are to be connected, whence they vary more than thoſe of the firſt diviſion. Theſe are the parts for progreſſive motion, the organs of ſenſe and

and the organs of digeſtion; all which either act, or are acted upon, by external matter.

This variation from external cauſes in many inſtances influences the ſhape of the whole, or particular parts, even giving a peculiar form to ſome which belong to the firſt order of actions, as the heart, which in this tribe, in the Seal, Otter, &c. is flattened, becauſe the cheſt is flattened for the purpoſe of ſwimming. The contents of the abdomen are not only adapted to the external form; but their direction in the cavity is, in ſome inſtances, regulated by it. The anterior extremity, or fin, although formed of diſtinct parts, in ſome degree ſimilar to the anterior extremities of ſome quadrupeds, being compoſed of ſimilar bones placed nearly in the ſame manner, yet are ſo formed and arranged as to fit them for progreſſive motion in the water only.

The external form of this order of animals is ſuch as fits them for dividing the water in progreſſive motion, and gives them power to produce that motion in the ſame manner as thoſe fiſh which move with a conſiderable velocity. On account of their inhabiting the water, their external form is more uniform than in animals of the ſame claſs which live upon land, the ſurface of the earth on which the progreſſive motion of the quadruped is to be performed being various and irregular, while the water is always the ſame.

The form of the head or anterior part of this order of animals is commonly a cone, or an inclined plane, except in the Spermaceti Whale, in which it terminates in a blunt ſurface. This form of head increaſes the ſurface of contact to the ſame volume of water which it removes, leſſens the preſſure, and is better calculated to bear the reſiſtance of the water through which the animal is to paſs; probably, on this account, the head

is larger than in quadrupeds, having more the proportion obſerved in fiſh, and ſwelling out laterally at the articulation of the lower jaw: this may probably be for the better catching their prey, as they have no motion of the head on the body; and this diſtance between the articulations of the jaw is ſomewhat ſimilar to the Swallow, Goat-ſucker, Bat, &c. which may alſo be accounted for, from their catching their food in the ſame manner as fiſh; and this is rendered ſtill more probable, ſince the form of the mouth varies according as they have or have not teeth. There is, however, in the Whale tribe more variety in the form of the head than of any other part, as in the Whalebone, Bottle-noſe, and Spermaceti Whales; though in this laſt it appears to owe its ſhape, in ſome ſort, to the vaſt quantity of ſpermaceti lodged there, and not to be formed merely for the catching of its prey. From the mode of their progreſſive motion, they have not the connection between the head and body, that is called the neck, as that would have produced an inequality inconvenient to progreſſive motion.

The body behind the fins or ſhoulders diminiſhes gradually to the ſpreading of the tail; but the part beyond the opening of the anus is to be conſidered as tail, although to appearance it is a continuation of the body. The body itſelf is flattened laterally; and, I believe, the back is much ſharper than the belly.

The projecting part, or tail, contains the power that produces progreſſive motion, and moves the broad termination, the motion of which is ſimilar to that of an oar in ſculling a boat; it ſuperſedes the neceſſity of poſterior extremities, and allows of the proper ſhape for ſwimming; that the form may be preſerved as much as poſſible, we find that all the projecting

parts, found in land animals of the ſame claſs, are either intirely wanting, as the external ear; are placed internally, as the teſticles; or are ſpread along under the ſkin, as the udder.

The tail is flattened horizontally, which is contrary to that of fiſh, this poſition of tail giving the direction to the animal in the progreſſive motion of the body. I ſhall not purſue this circumſtance further than to apply it to thoſe purpoſes in the animal oeconomy, for which this particular direction is intended.

The two lateral fins, which are analogous to the anterior extremities in the quadruped, are commonly ſmall, varying however in ſize, and ſeem to ſerve as a kind of oars.

To aſcertain the uſe of the *fin* on the back is probably not ſo eaſy, as the large Whalebone and Spermaceti Whales have it not; one ſhould otherwiſe conceive it intended to preſerve the animal from turning.

I believe, like moſt animals, they are of a lighter colour on their belly than on their back: in ſome they are intirely white on the belly; and this white colour begins by a regular determined line, as in the Grampus, Piked Whale, &c.: in others, the white on the belly is gradually ſhaded into the dark colour of the back, as in the Porpoiſe. I have been informed, that ſome of them are pied upwards and downwards, or have the diviſions of colour in a contrary direction.

The element in which they live renders certain parts which are of importance in other animals uſeleſs in them, gives to ſome parts a different action, and renders others of leſs account.

The puncta lachrymalia with the appendages, as the ſac and duct, are in them unneceſſary; and the ſecretion from the lachrymal gland is not water, but mucus, as it alſo is in the Turtle; and

and we may ſuppoſe only in ſmall quantity, the gland itſelf being ſmall.

The urinary bladder is ſmaller than in quadrupeds; and indeed there is not any apparent reaſon why Whales ſhould have one at all.

The tongue is flat, and but little projecting, as they neither have voice, nor require much action of this part, in applying the food between the teeth for the purpoſe of maſtication, or deglutition, being nearly ſimilar to fiſh in this reſpect, as well as in their progreſſive motion.

In ſome particulars they differ as much from one another as any two genera of quadrupeds I am acquainted with.

The larynx, ſize of trachea, and number of ribs, differ exceedingly. The cæcum is only found in ſome of them. The teeth in ſome are wanting. The blow-holes are two in number in many, in others only one. The whalebone and ſpermaceti are peculiar to particular genera: all which conſtitute great variations. In other reſpects we find an uniformity, which would appear to be independent of their living and moving only in the water, as in the ſtomach, liver, parts of generation of both ſexes, and in the kidneys: in theſe laſt however, I believe, it depends in ſome degree upon their ſituation, although it is extended to other animals, the cauſe of which I do not underſtand.

All animals have, I believe, a ſmell peculiar to themſelves: how far this is connected with the other diſtinctions, I do not know, our organs not being able to diſtinguiſh with ſufficient accuracy.

The ſmell of animals of this tribe is the ſame with that of the Seal, but not ſo ſtrong, a kind of ſour ſmell, which the

Seal has while alive; the oil has the ſame ſmell with that of the Salmon, Herring, Sprat, &c.

The obſervations reſpecting the weight of the fleſh of animals that ſwim, which I publiſhed in my obſervations on the œconomy of certain parts of animals, are applicable to theſe alſo; for the fleſh in this tribe is rather heavier than beef; two portions of muſcle of the ſame ſhape, one from the pſoas muſcle of the Whale, the other of an ox, when weighed in air, were both exactly 502 grains; but, weighed in water, the portion of the Whale was four grains heavier than the other. It is probable, therefore, that the neceſſary equilibrium between the water and the animal is produced by the oil, in addition to which the principal action of the tail is ſuch as tends either to raiſe them, or keep them ſuſpended in the water, according to the degree of force with which it acts.

From the tail being horizontal, the motion of the animal, when impelled by it, is up and down: two advantages are gained by this, it gives the neceſſary opportunity of breathing, and elevates them in the water; for every motion of the tail tends, as I ſaid before, to raiſe the animal: and that this may be effected, the greateſt motion of the tail is downwards, thoſe muſcles being very large, making two ridges in the abdomen; this motion of the tail raiſes the anterior extremity, which always tends to keep the body ſuſpended in the water.

Of the Bones.

The bones alone, in many animals, when properly united into what is called the ſkeleton, give the general ſhape and character of the animal. Thus a quadruped is diſtinguiſhed from a bird, and even one quadruped from another, it only requiring

requiring a skin to be thrown over the skeleton to make the species known; but this is not so decidedly the case with this order of animals, for the skeleton in them does not give us the true shape. An immense head, a small neck, few ribs, and in many a short sternum, and no pelvis, with a long spine, terminating in a point, require more than a skin being laid over them to give the regular and characteristic form of the animal.

The bones of the anterior extremity give no idea of the shape of a fin, the form of which depends wholly upon its covering. The different parts of the skeleton, are so inclosed, and the spaces between the projecting parts are so filled up, as to be altogether concealed, giving the animal externally an uniform and elegant form, resembling an insect enveloped in its chrysalis coat.

The bones of the head are in general so large, as to render the cavity which contains the brain but a small part of the whole; while, in the human species, and in birds, this cavity constitutes the principal bulk of the head. This is, perhaps, most remarkable in the Spermaceti Whale; for on a general view of the bones of the head, it is impossible to determine where the cavity of the skull lies, till led to it by the foramen magnum occipitale. The same remark is applicable to the large Whalebone and Bottle-nose Whale; but in the Porpoise, where the brain is larger in proportion to the size of the animal, the skull makes the principal part of the head.

Some of the bones in one genus differ from those of another. The lower jaw is an instance of this. In the Spermaceti and Bottle-nose Whales, the Grampus, and the Porpoise, the lower jaws, especially at the posterior ends, resemble each other;

other; but in both the large and ſmall Whalebone Whales, the ſhape differs conſiderably. The number of ſome particular bones varies likewiſe very much.

The Piked Whale has ſeven vertebræ in the neck, twelve which may be reckoned to the back, and twenty-ſeven to the tail, making forty-ſix in the whole.

In the porpoiſe there are five cervical vertebræ, and one common to the neck and back, fourteen proper to the back, and 30 to the tail, making in the whole fifty-one.

The ſmall Bottle-noſe Whale, caught near Berkeley, in the number of cervical vertebræ reſembled the Porpoiſe; it had ſeventeen in the back, and thirty-ſeven in the tail, in all ſixty.

In the Porpoiſe, four of the vertebræ of the neck are anchyloſed; and in every animal of this order, which I have examined, the atlas is by much the thickeſt, and ſeems to be made up of two joined together, for the ſecond cervical nerve paſſes through a foramen in this vertebra. There is no articulation for rotatory motion between the firſt and ſecond vertebræ of the neck.

The ſmall Bottle-noſe Whale had eighteen ribs on each ſide, the Porpoiſe ſixteen. The ends of the ribs that have two articulations, in the whole of this tribe, I believe, are articulated with the body of the vertebræ above, and with the tranſverſe proceſſes below, by the angles; ſo that there is one vertebra common to the neck and back. In the large Whalebone Whale the firſt rib is bifurcated, and conſequently articulated to two vertebræ.

The ſternum is very flat in the Piked Whale; it is only one very ſhort bone; and in the Porpoiſe it is a good deal longer. In the ſmall Bottle-noſe it is compoſed of three

bones, and is of ſome length. In the Piked Whale the firſt rib, and in the Porpoiſe the three firſt, are articulated with the ſternum.

As a contraction, correſponding to the neck in quadrupeds, would have been improper in this order of animals, the vertebræ of the neck are thin, to make the diſtance between the head and ſhoulders as ſhort as poſſible, and in the ſmall Bottle-noſe Whale are only ſix in number.

The ſtructure of the bones is ſimilar to that of the bones of quadrupeds; they are compoſed of an animal ſubſtance, and an earth that is not animal: theſe ſeem only to be mechanically mixed, or rather the earth thrown into the interſtices of the animal part. In the bones of fiſhes this does not ſeem to be the caſe, the earth in many fiſh being ſo united with the animal part, as to render the whole tranſparent, which is not the caſe when the animal part is removed by ſteeping the bone in cauſtic alkali: nor is the animal part ſo tranſparent when deprived of the earth. The bones are leſs compact than thoſe of quadrupeds that are ſimilar to them.

Their form ſomewhat reſembles what takes place in the quadruped, at leaſt in thoſe whoſe uſes are ſimilar, as the vertebræ, ribs, and bones of the anterior extremities have their articulations in part alike, although not in all of them. The articulation of the lower jaw, of the carpus, metacarpus, and fingers, are exceptions. The articulation of the lower jaw is not by ſimple contact either ſingle or double, joined by a capſular ligament, as in the quadruped; but by a very thick intermediate ſubſtance of the ligamentous kind, ſo interwoven that its parts move on each other, in the interſtices of which is an oil. This thick matted ſubſtance may anſwer the ſame purpoſe as the double joint in the quadruped.

The

The two fins are analogous to the anterior extremities of the quadruped, and are alſo ſomewhat ſimilar in conſtruction. A fin is compoſed of a ſcapula, os humeri, ulna, radius, carpus, and metacarpus, in which laſt may be included the fingers, becauſe the number of bones are thoſe which might be called fingers, although they are not ſeparated, but included in one general covering with the metacarpus. They have nothing analogous to the thumb, and the number of bones in each is different; in the fore-finger there are five bones, in the middle and ring-finger ſeven, and in the little finger four. The articulation of the carpus, metacarpus, and fingers, is different from that of the quadruped, not being by capſular ligament, but by intermediate cartilages connected to each bone. Theſe cartilages between the different bones of the fingers are of conſiderable length, being nearly equal to one-half of that of the bone; and this conſtruction of the parts gives firmneſs, with ſome degree of pliability, to the whole.

As this order of animals cannot be ſaid to have a pelvis, they of courſe have no os ſacrum, and therefore the vertebræ are continued on to the end of the tail; but with no diſtinction between thoſe of the loins and tail. But as thoſe vertebræ alone would not have had ſufficient ſurface to give riſe to the muſcles requiſite to the motion of the tail, there are bones added to the fore-part of ſome of the firſt vertebræ of the tail, ſimilar to the ſpinal proceſſes on the poſterior ſurface.

From all theſe obſervations we may infer, that the ſtructure, formation, arrangement, and the union of the bones, which compoſe the forms of parts in this order of animals, are much upon the ſame principle as in quadrupeds.

The fleſh or muſcles of this order of animals is red, reſembling that of moſt quadrupeds, perhaps more like that of the

Bull or Horſe than any other animal: ſome of it is very firm; and about the breaſt and belly it is mixed with tendon.

Although the body and tail is compoſed of a ſeries of bones connected together and moved as in fiſh, yet it has its movements produced by long muſcles, with long tendons, which renders the body thicker, while the tail at its ſtem is ſmaller than that of any other ſwimmer, whoſe principal motion is the ſame. Why this mode of applying the moving powers ſhould not have been uſed in fiſh, is probably not ſo eaſily anſwered; but in fiſh the muſcles of the body are of nearly the ſame length as the vertebræ.

The depreſſor muſcles of the tail, which are ſimilar in ſituation to the pſoæ, make two very large ridges on the lower part of the cavity of the belly, riſing much higher than the ſpine, and the lower part of the aorta paſſes between them.

Theſe two large muſcles, inſtead of being inſerted into two extremities as in the quadruped, go to the tail, which may be conſidered in this order of animals as the two poſterior extremities united into one.

Their muſcles, a very ſhort time after death, loſe their fibrous ſtructure, become as uniform in texture as clay or dough, and even ſofter. This change is not from putrefaction, as they continue to be free from any offenſive ſmell, and is moſt remarkable in the pſoæ muſcles, and thoſe of the back.

Of the Conſtruction of the Tail.

The mode in which the tail is conſtructed is, perhaps, as beautiful, as to the mechaniſm, as any part of the animal. It is wholly compoſed of three layers of tendinous fibres, covered by the common cutis and cuticle: two of theſe layers

are external, the other internal. The direction of the fibres of the external layers is the ſame as in the tail, forming a ſtratum about one-third of an inch thick; but varying, in this reſpect, as the tail is thicker or thinner. The middle layer is compoſed entirely of tendinous fibres, paſſing directly acroſs, between the two external ones above deſcribed, their length being in proportion to the thickneſs of the tail; a ſtructure which gives amazing ſtrength to this part.

The ſubſtance of the tail is ſo firm and compact, that the veſſels retain their dilated ſtate, even when cut acroſs; and this ſection conſiſts of a large veſſel ſurrounded by as many ſmall ones as can come in contact with its external ſurface; which of theſe are arteries, and which veins, I do not know.

The fins are merely covered with a ſtrong condenſed adipoſe membrane.

Of the Fat.

The fat of this order of animals, except the ſpermaceti, is what we generally term oil. It does not coagulate in our atmoſphere, and is probably the moſt fluid of animal fats; but the fat of every different order of animals has not a peculiar degree of ſolidity, ſome having it in the ſame ſtate, as the Horſe and Bird. What I believe approaches neareſt to ſpermaceti, is the fat of ruminating animals, called tallow.

The fat is differently ſituated in different orders of animals; probably for particular purpoſes, at leaſt in ſome we can aſſign a final intention. In the animals, which are the ſubject of the preſent Paper, it is found principally on the outſide of the muſcles, immediately under the ſkin, and is in conſiderable quantity. It is rarely to be met with in the interſtices of the muſcles, or in any of the cavities, ſuch as the abdomen or about the heart.

In

In animals of the ſame claſs living on land, the fat is more diffuſed: it is ſituated, more eſpecially when old, in the interſtices of muſcles, even between the faſciculi of muſcular fibres, and is attached to many of the viſcera; but many parts are free from fat, unleſs when diſeaſed, as the penis, ſcrotum, teſticle, eyelid, liver, lungs, brain, ſpleen, &c.

In fiſh its ſituation is rather particular, and is moſt commonly in two modes; in the one, diffuſed through the whole body of the fiſh, as in the Salmon, Herring, Pilchard, Sprat, &c.; in the other, it is found in the liver only, as in all of the Ray kind, Cod, and in all thoſe called White-fiſh, there being none in any other part of the body *. The fat of fiſh appears to be diffuſed through the ſubſtance of the parts which contain it, but is probably in diſtinct cells. In ſome of theſe fiſh, where it is diffuſed over the whole body, it is more in ſome parts than others, as on the belly of the Salmon, where it is in larger quantity.

The fat is differently incloſed in different orders of animals. In the quadruped, thoſe of the Seal kind excepted, in the bird, amphibia, and in ſome fiſh, it is contained in looſe cellular membrane, as if in bags, compoſed of ſmaller ones, by which means the larger admit of motion on one another, and on their connecting parts; which motion is in a greater or leſs degree, as is proper or uſeful. Where motion could anſwer no purpoſe, as in the bones, it is confined in ſtill ſmaller cells. The fat is in a leſs degree in the ſoles of the feet, palms of the hands, and in the breaſts of many animals. In this order of animals and the Seal kind, as far as I yet know, it is diſpoſed of in two ways; the ſmall quantity found in the cavities of the body,

* The Sturgeon is, however, an exception, having its fat in particular ſituations, and in the interſtices of parts, as in other animals.

and

and interſtices of parts, is in general diſpoſed in the ſame way as in quadrupeds; but the external, which includes the principal part, is incloſed in a reticular membrane, apparently compoſed of fibres paſſing in all directions, which ſeem to confine its extent, allowing it little or no motion on itſelf, the whole, when diſtended, forming almoſt a ſolid body. This, however, is not always the caſe in every part of animals of this order; for under the head, or what may be rather called neck, of the Bottle-noſe, the fat is confined in larger cells, admitting of motion. This reticular membrane is very fine in ſome, and very ſtrong and coarſe in others, and even varies in different parts of the ſame animal. It is fine in the Porpoiſe, Spermaceti, and large Whalebone Whale; coarſe in the Grampus and ſmall Whalebone Whale*: in all of them it is fineſt on the body, becoming coarſer towards the tail, which is compoſed of fibres without any fat: which is alſo the caſe in the covering of the fins. This reticular net-work in the Seal is very coarſe; and in thoſe which are not fat, when it collapſes, it looks almoſt like a fine net with ſmall meſhes. This ſtructure confines the animal to a determined ſhape, whereas in quadrupeds fat when in great quantity deſtroys all ſhape.

The fat differs in conſiſtence in different animals, and in different parts of the ſame animal, in which its ſituation is various. In quadrupeds, ſome have the external fat ſofter than the internal; and that incloſed in bones is ſofteſt nearer to their extremities. Ruminating animals have that ſpecies of fat called tallow, and in their bones they have either hard fat or marrow, or fluid fat called Neat's-foot oil. In this order of animals, the internal fat is the leaſt fluid, and is nearly of the conſiſtence of Hog's-lard; the

* Where it is fine, it yields the largeſt quantity of oil, and requires the leaſt boiling.

external is the common train oil; but the Spermaceti Whale differs from every other animal I have examined, having the two kinds of fat juſt mentioned, and another which is totally different, called ſpermaceti, of which I ſhall give a particular account.

What is called ſpermaceti is found every where in the body in ſmall quantity, mixed with the common fat of the animal, bearing a very ſmall proportion to the other fat. In the head it is the reverſe, for there the quantity of ſpermaceti is large when compared to that of the oil, although they are mixed, as in the other parts of the body.

As the ſpermaceti is found in the largeſt quantity in the head, and in what would appear on a ſlight view to be the cavity of the ſkull, from a peculiarity in the ſhape of that bone, it has been imagined by ſome to be the brain.

Theſe two kinds of fat in the head are contained in cells, or cellular membrane, in the ſame manner as the fat in other animals; but beſides the common cells there are larger ones, or ligamentous partitions going acroſs, the better to ſupport the vaſt load of oil, of which the bulk of the head is principally made up.

There are two places in the head where this oil lies; theſe are ſituated along its upper and lower part: between them paſs the noſtrils, and a vaſt number of tendons going to the noſe and different parts of the head.

The pureſt ſpermaceti is contained in the ſmalleſt and leaſt ligamentous cells: it lies above the noſtril, all along the upper part of the head, immediately under the ſkin, and common adipoſe membrane. Theſe cells reſemble thoſe which contain the common fat in the other parts of the body neareſt the ſkin. That which lies above the roof of the mouth, or between it

and

and the noſtril, is more intermixed with a ligamentous cellular membrane, and lies in chambers whoſe partitions are perpendicular. Theſe chambers are ſmaller the nearer to the noſe, becoming larger and larger towards the back part of the head, where the ſpermaceti is more pure.

This ſpermaceti, when extracted cold, has a good deal the appearance of the internal ſtructure of a water melon, and is found in rather ſolid lumps.

About the noſe, or anterior part of the noſtril, I diſcovered a great many veſſels, having the appearance of a plexus of veins, ſome as large as a finger. On examining them, I found they were loaded with the ſpermaceti and oil; and that ſome had correſponding arteries. They were moſt probably lymphatics; therefore I ſhould ſuppoſe, that their contents had been abſorbed from the cells of the head. We may the more readily ſuppoſe this, from finding many of the cells, or chambers, almoſt empty; and as we may reaſonably believe that this animal had been ſome time out of the ſeas in which it could procure proper food, it had perhaps lived on the ſuperabundance of oil.

The ſolid maſſes are what are brought home in caſks for ſpermaceti.

I found, by boiling this ſubſtance, that I could eaſily extract the ſpermaceti and oil which floated on the top from the cellular membrane. When I ſkimmed off the oily part, and let it ſtand to cool, I found that the ſpermaceti cryſtalliſed, and the whole became ſolid; and by laying this cake upon any ſpongy ſubſtance, as chalk, or on a hollow body, the oil drained all off, leaving the ſpermaceti pure and white. Theſe cryſtals were only attached to each other by edges, forming a ſpongy maſs; and by melting this pure ſpermaceti, and allowing it to cry-

ſtalliſe, it was reduced in appearance to half its bulk, the cryſtals being ſmaller, and more blended, conſequently leſs diſtinct.

The ſpermaceti mixes readily with other oils, while it is in a fluid ſtate, but ſeparates or cryſtalliſes whenever it is cooled to a certain degree; like two different ſalts being diſſolved in water, one of which will cryſtalliſe with a leſs degree of evaporation than the other; or, if the water is warm, and fully ſaturated, one of the ſalts will cryſtalliſe ſooner than the other, while the ſolution is cooling. I wanted to ſee whether ſpermaceti mixed equally well with the expreſſed oils of vegetables when warm, and likewiſe ſeparated and cryſtalliſed when cold, and on trial there ſeemed to be no difference. When very much diluted with the oil, it is diſſolved or melted by a much ſmaller degree of heat than when alone; and this is the reaſon, perhaps, that it is in a fluid ſtate in the living body.

If the quantity of ſpermaceti is ſmall in proportion to the other oil, it is, perhaps, nearly in that proportion longer in cryſtalliſing; and when it does cryſtalliſe, the cryſtals are much ſmaller than thoſe that are formed where the proportion of ſpermaceti is greater. From the ſlowneſs with which the ſpermaceti cryſtalliſes when much diluted with its oil, from a conſiderable quantity being to be obtained in that way, and from its continuing for years to cryſtalliſe, one would be induced to think, that perhaps the oil itſelf is converted into ſpermaceti.

It is moſt likely, that if we could diſcover the exact form of the different cryſtals of oils, we ſhould thence be able to aſcertain both the different ſorts of vegetable oils, expreſſed and eſſential, and the different ſorts of animal oils, much better than by any other means; in the ſame manner as we know ſalts by the forms into which they ſhoot.

The ſpermaceti does not become rancid, or putrid, nearly ſo ſoon as the other animal oils; which is moſt probably owing to the ſpermaceti being for the moſt part in a ſolid ſtate; and I ſhould ſuppoſe, that few oils would become ſo ſoon rancid as they do, if they were always preſerved in that degree of cold which rendered them ſolid: neither does this oil become ſo ſoon putrid as the fleſh of the animal; and therefore, although the oil in the cells appeared to be putrid before boiling, it was ſweet when deprived of the cellular ſubſtance. The ſpermaceti is rather heavier than the other oil.

In this animal then we find two ſorts of oil, beſides the deeper ſeated fat, common to all of this claſs; one of which cryſtalliſes with a much leſs degree of cold than the other, and of courſe requires a greater degree of heat to melt it, and forms, perhaps, the largeſt cryſtals of any expreſſed oil we know: yet the fluid oil of this animal will cryſtalliſe in an extreme hard froſt, much ſooner than moſt eſſential oils, though not ſo ſoon as the expreſſed oils of vegetables. Camphire, however, is an exception, ſince it cryſtalliſes in our warmeſt weather, and when melted with expreſſed oil of vegetables, if the oil is too much ſaturated for that particular degree of cold, cryſtalliſes exactly like ſpermaceti.

In the Ox the tallow, and what is called Neat's-foot oil, cryſtalliſe in different degrees of cold. The tallow congeals with rather leſs cold than the ſpermaceti; but the other oil is ſimilar to what is called the train oil in the Whale.

I have endeavoured to diſcover the form of the cryſtals of different ſorts of oil; but could never determine exactly what that was, becauſe I could never find any of the cryſtals ſingle, and by being always united, the natural form was not diſtinct.

It is the adipoſe covering from all of the Whale kind that is brought home in ſquare pieces, called flitches, and which, by being boiled, yields the oil on expreſſion, leaving the cellular membrane. When theſe flitches have become in ſome degree putrid, there iſſues two ſorts of oil; the firſt is pure, the laſt ſeems incorporated with part of the animal ſubſtance, which has become eaſy of ſolution from its putridity, forming a kind of butter. It is unctuous to the touch, ropy, coagulates or becomes harder by cold, ſwims upon water, not being ſoluble in it; and the pure oil, ſeparating in the ſame manner from this, ſwims above all.

What remains, after all the oil is extracted, retains a good deal of its form, is almoſt wholly convertible into glue, and is ſold for that purpoſe.

The cellular, or rather what ſhould be called the uniting membrane in this order of animals, is ſimilar to that in the quadruped; we find it uniting muſcle to muſcle, and muſcle to bone, for their eaſy motion on one another.

The cellular membrane, which is the receptacle for the oil near the ſurface of the body is in general very different from that in the quadruped, as has been already obſerved.

Of the Skin.

The covering of this order of animals conſiſts of a cuticle and cutis.

The cuticle is ſomewhat ſimilar to that on the ſole of the foot in the human ſpecies, and appears to be made up of a number of layers, which ſeparate by ſlight putrefaction; but this I ſuſpect ariſes in ſome degree from there being a ſucceſſion of cuticles formed. It has no degree of elaſticity or toughneſs,

but tears easily; nor do its fibres appear to have any particular direction. The internal stratum is tough and thick, and in the Spermaceti Whale its internal surface, when separated from the cutis, is just like coarse velvet, each pile standing firm in its place; but this is not so distinguishable in some of the others, although it appears rough from the innumerable perforations.

It is the cuticle that gives the colour to the animal; and in parts that are dark, I think, I have seen a dirty coloured substance washed away in the separation of the cuticle from the cutis, which must be a kind of rete mucosum.

The cutis in this tribe is extremely villous on its external surface, answering to the rough surface of the cuticle, and forming in some parts small ridges, similar to those on the human fingers and toes. These villi are soft and pliable; they float in water, and each is longer or shorter according to the size of the animal. In the Spermaceti Whale they were about a quarter of an inch long; in the Grampus, Bottle-nose and Piked Whales, much shorter; in all, they are extremely vascular.

The cutis seems to be the termination of the cellular membrane of the body more closely united, having smaller interstices, and becoming more compact. This alteration in the texture is so sudden as to make an evident distinction between what is solely connecting membrane, and skin, and is most evident in lean animals; for in the change from fat to lean, the skin does not undergo an alteration equal to what takes place in the adipose membrane, although it may be observed, that the skin itself is diminished in thickness. In fat animals the distinction between skin and cellular membrane is much less, the gradation from the one to the other seeming to be slower; for the cells of both membrane and skin being loaded with fat, the whole

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has more the appearance of one uniform ſubſtance. This uniformity of the adipoſe membrane and ſkin is moſt obſervable in the Whale, Seal, Hog, and the human ſpecies; and is not only viſible in the raw but in the dreſſed hides; for in dreſſed ſkins the external is much more compact in texture than the inner ſurface, and is in common very tough.

In ſome animals the cutis is extremely thick, and in ſome parts much more ſo than others: where very thick, it appears to be intended as a defence againſt the violence of their own ſpecies or other animals. In moſt quadrupeds it is muſcular, contracting by cold, and relaxing by heat. Many other ſtimulating ſubſtances make it contract; but cold is probably that ſtimulus by which it was intended to be generally affected.

The ſkin is extremely elaſtic in the greateſt number of quadrupeds, and in its contracted ſtate may be ſaid to be rather too ſmall for the body; by this elaſticity it adapts itſelf to the changes which are conſtantly taking place in the parts, and it is from the want of it, that it becomes too large in ſome old animals. In all animals it is more elaſtic in ſome parts than others, eſpecially in thoſe where there is the greateſt motion. How far theſe variations take place in the Whale I do not exactly know; but a looſe elaſtic ſkin in this tribe would appear to be improper as an univerſal covering, conſidering the progreſſive motion of the animal, and the medium in which it moves; therefore it appears to be kept always on the ſtretch, by the adipoſe membrane being loaded with fat, which does not allow the ſkin to recede when cut. It is, however, more elaſtic at the ſetting on of the eyelids, round the opening of the prepuce, the nipples, the ſetting on of the fins, and under the jaw, to allow of motion in thoſe parts; and

and here there is more reticular, and leſs adipoſe membrane. But in the Piked Whale there is probably one of the moſt ſtriking inſtances of an elaſtic cuticular contraction: for though the whole ſkin of the fore part of the neck and breaſt of the animal, as far down as the middle of the belly, be extremely elaſtic; yet to render it ſtill more ſo, it is ribbed longitudinally like a ribbed ſtocking, which gives an increaſed lateral elaſticity. Theſe ribs are, when contracted, about five-eighths of an inch broad, covered with the common ſkin of the animal; but in the hollow part of the rib, it is of a ſofter texture, with a thinner cuticle. This part is poſſeſſed of the greateſt elaſticity; why it ſhould be ſo elaſtic is difficult to ſay, as it covers the thorax, which can never be increaſed in ſize; yet there muſt be ſome peculiar circumſtance in the œconomy of the ſpecies requiring this ſtructure, which we as yet know nothing of.

The ſkin is intended for various purpoſes. It is the univerſal covering given for the defence of all kinds of animals; and that it might anſwer this purpoſe well, it is the ſeat of one of the ſenſes.

Of the Mode of catching their Food.

The mouths of animals are the firſt parts to be conſidered reſpecting nouriſhment or food, and are ſo much connected with every thing relative to it, as not only to give good hints whether the food is vegetable or animal, but alſo reſpecting the particular kind of either, eſpecially of animal food. The mouth not only receives the food, but is the immediate inſtrument for catching it. As it is a compound inſtrument in many animals, having parts of various conſtructions belonging to it, I ſhall at preſent conſider it in this tribe no further than as connected

connected with their mode of catching the food, and adapting and diſpoſing it for being ſwallowed. It is probable, that theſe animals do not require either a diviſion of the food, or a maſtication of it in the mouth, but ſwallow whatever they catch, whole; for we do not find any of them furniſhed with parts capable of producing either effect. The mouth in moſt of this tribe is well adapted for catching the food; the jaws ſpread as they go back, making the mouth proportionally wider than in many other animals.

There is a very great variety in the formation of the mouths of this tribe of animals, which we have many opportunities of knowing, from the head being often brought home when the other parts of the animal are rejected; a circumſtance which frequently leaves us ignorant of the particular ſpecies to which it belonged.

Some catch their food by means of teeth, which are in both jaws, as the Porpoiſe and Grampus; in others, they are only in one jaw, as in the Spermaceti Whale; and in the large Bottle-noſe Whale, deſcribed by DALE, there are only two ſmall teeth in the anterior part of the lower jaw. In the Narwhale only two tuſks in the fore part of the upper jaw *; while in ſome others there are none at all. In thoſe which have teeth in both jaws, the number in each varies conſiderably; the ſmall Bottle-noſe had forty-ſix in the upper, and fifty in the lower; and in the jaws of others there are only five or ſix in each.

The teeth are not diviſible into different claſſes, as in quadrupeds; but are all pointed teeth, and are commonly a good deal ſimilar. Each tooth is a double cone, one point being

* I call theſe tuſks to diſtinguiſh them from common teeth. A tuſk is that kind of tooth which has no bounds ſet to its growth, excepting by abraſion, as the tuſk of the Elephant, Boar, Sea-horſe, Manatee, &c.

faſtened

fastened in the gum, the other projecting: they are, however, not all exactly of this shape. In some species of Porpoise the fang is flattened, and thin at its extremity; in the Spermaceti Whale the body of the tooth is a little curved towards the back part of the mouth; this is also the case in some others. The teeth are composed of animal substance and earth, similar to the bony part of the teeth in quadrupeds. The upper teeth are commonly worn down upon the inside, the lower on the outside; this arises from the upper jaw being in general the largest.

The situation of the teeth, when first formed, and their progress afterwards, as far as I have been able to observe, is very different in common from those of the quadruped. In the quadruped the teeth are formed in the jaw, almost surrounded by the alveoli, or sockets, and rise in the jaw as they increase in length; the covering of the alveoli being absorbed, the alveoli afterwards rise with the teeth, covering the whole fang; but in this tribe the teeth appear to form in the gum, upon the edge of the jaw, and they either sink in the jaw as they lengthen, or the alveoli rise to inclose them: this last is most probable, since the depth of the jaw is also increased, so that the teeth appear to sink deeper and deeper in the jaw. This formation is readily discovered in jaws not full grown; for the teeth increase in number as the jaw lengthens, as in other animals. The posterior part of the jaw becoming longer, the number of teeth in that part increases, the sockets becoming shallower and shallower, and at last being only a slight depression.

It would appear, that they do not shed their teeth, nor have they new ones formed similar to the old, as is the case with most other quadrupeds, and also with the Alligator. I have never been able to detect young teeth under the roots of the old ones; and indeed the situation in which they are first formed

makes it in ſome degree impoſſible, if the young teeth follow the ſame rule in growing with the original ones, as they probably do in moſt animals.

If it is true, that the Whale tribe do not ſhed their teeth, in what way are they ſupplied with new ones, correſponding in ſize with the increaſed ſize of the jaw? It would appear, that the jaw, as it increaſes poſteriorly, decays at the ſymphyſis, and while the growth is going on, there is a conſtant ſucceſſion of new teeth, by which means the new-formed teeth are proportioned to the jaw. The ſame mode of growth is evident in the Elephant, and in ſome degree in many fiſh; but in theſe laſt the abſorption of the jaw is from the whole of the outſide along where the teeth are placed. The depth of the alveoli ſeems to prove this, being ſhallow at the back part of the jaw, and becoming deeper towards the middle, where they are the deepeſt, the teeth there having come to the full ſize. From this forwards they are again becoming ſhallower, the teeth being ſmaller, the ſockets waſting, and at the ſymphyſis there are hardly any ſockets at all. This will make the exact number of teeth in any ſpecies uncertain.

Some genera of this tribe have another mode of catching their food, and retaining it till ſwallowed, which is by means of the ſubſtance called Whalebone. Of this there are two kinds known; one very large, probably from the largeſt Whale yet diſcovered; the other from a ſmaller ſpecies.

This whalebone, which is placed on the inſide of the mouth, and attached to the upper jaw, is one of the moſt ſingular circumſtances belonging to this ſpecies, as they have moſt other parts in common with quadrupeds. It is a ſubſtance, I believe, peculiar to the Whale, and of the ſame nature as horn, which I ſhall uſe as a term to expreſs what conſti-

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tutes hair, nails, claws, feathers, &c. it is wholly compoſed of animal ſubſtance, and extremely elaſtic *.

Whalebone conſiſts of thin plates of ſome breadth, and in ſome of very conſiderable length, their breadth and length in ſome degree correſponding to one another; and when longeſt they are commonly the broadeſt, but not always ſo. (See Tab. XXII.) Theſe plates are very different in ſize in different parts of the ſame mouth, more eſpecially in the large Whalebone Whale, whoſe upper jaw does not paſs parallel upon the under, but makes an arch, the ſemidiameter of which is about one-fourth of the length of the jaw. The head in my poſſeſſion is nineteen feet long, the ſemidiameter not quite five feet: if this proportion is preſerved, thoſe Whales which have whalebone fifteen feet long muſt be of an immenſe ſize.

Theſe plates are placed in ſeveral rows, encompaſſing the outer ſkirts of the upper jaw, ſimilar to teeth in other animals. They ſtand parallel to each other, having one edge towards the circumference of the mouth, the other towards the center or cavity. They are placed near together in the Piked Whale, not being a quarter of an inch aſunder where at the greateſt diſtance, yet differing in this reſpect in different parts of the ſame mouth; but in the great Whale the diſtances are more conſiderable.

The outer row is compoſed of the longeſt plates; and theſe are in proportion to the different diſtances between the two jaws, ſome being fourteen or fifteen feet long, and twelve or fifteen inches broad; but towards the anterior and poſterior part of the mouth, they are very ſhort: they riſe for half a foot or more, nearly of equal breadths, and afterwards ſhelve off from their inner ſide until they come near to a point at the

* From this it muſt appear, that the term bone is an improper one.

outer: the exterior of the inner rows are the longeſt, corresponding to the termination of the declivity of the outer, and become ſhorter and ſhorter till they hardly riſe above the gum.

The inner rows are cloſer than the outer, and riſe almoſt perpendicularly from the gum, being longitudinally ſtraight, and have leſs of the declivity than the outer. The plates of the outer row laterally are not quite flat, but make a ſerpentine line, more eſpecially in the Piked Whale the outer edge is thicker than the inner. All round the line made by their outer edges, runs a ſmall white bead, which is formed along with the whalebone, and wears down with it. The ſmaller plates are nearly of an equal thickneſs upon both edges. In all of them, the termination is in a kind of hair, as if the plate was ſplit into innumerable ſmall parts, the exterior being the longeſt and ſtrongeſt.

The two ſides of the mouth compoſed of theſe rows meet nearly in a point at the tip of the jaw, and ſpread or recede laterally from each other as they paſs back; and at their poſterior ends, in the Piked Whale, they make a ſweep inwards, and come very near each other, juſt before the opening of the œſophagus. In the Piked Whale there were above three hundred in the outer rows on each ſide of the mouth. Each layer terminates in an oblique ſurface, which obliquity inclines to the roof of the mouth, anſwering to the gradual diminution of their length; ſo that the whole ſurface, compoſed of theſe terminations, forms one plane riſing gradually from the roof of the mouth; from this obliquity of the edge of the outer row, we may in ſome meaſure judge of the extent of the whole baſe, but not exactly, as it makes a hollow curve, which increaſes the baſe.

The

The whole ſurface reſembles the ſkin of an animal covered with ſtrong hair, under which ſurface the tongue muſt immediately lie, when the mouth is ſhut; it is of a light-brown colour in the Piked Whale, and is darker in the large Whale.

In the Piked Whale, when the mouth is ſhut, the projecting whalebone remains entirely on the inſide of the lower jaw, the two jaws meeting every where along their ſurface; but how this is effected in the large Whale I do not certainly know, the horizontal plane made by the lower jaw being ſtraight, as in the Piked Whale; but the upper jaw being an arch cannot be hid by the lower. I ſuppoſe, therefore, that a broad upper lip, meeting as low as the lower jaw, covers the whole of the outer edges of the exterior rows.

The whalebone is continually wearing down, and renewing in the ſame proportion, except when the animal is growing it is renewed faſter, and in proportion to the growth.

The formation of the whalebone is extremely curious, being in one reſpect ſimilar to that of the hair, horns, ſpurs, &c.; but it has beſides another mode of growth and decay, equally ſingular.

Theſe plates form upon a thin vaſcular ſubſtance, not immediately adhering to the jaw-bone; but having a more denſe ſubſtance between, which is alſo vaſcular. This ſubſtance, which may be called the nidus of the whalebone, ſends out (the above) thin broad proceſſes, anſwering to each plate, on which the plate is formed, as the Cock's ſpur or the Bull's horn, on the bony core, or a tooth on its pulp; ſo that each plate is neceſſarily hollow at its growing end, the firſt part of the growth taking place on the inſide of this hollow.

Beſides

Beſides this mode of growth, which is common to all ſuch ſubſtances, it receives additional layers on the outſide, which are formed upon the above-mentioned vaſcular ſubſtance extended along the ſurface of the jaw. This part alſo forms upon it a ſemi-horny ſubſtance between each plate, which is very white, riſes with the whalebone, and becomes even with the outer edge of the jaw, and the termination of its outer part forms the bead above mentioned. This intermediate ſubſtance fills up the ſpaces between the plates as high as the jaw, acts as abutments to the whalebone, or is ſimilar to the alveolar proceſſes of the teeth, keeping them firm in their places. (See Tab. XXIII.)

As both the whalebone and intermediate ſubſtance are conſtantly growing, and as we muſt ſuppoſe a determined length neceſſary, a regular mode of decay muſt be eſtabliſhed, not depending entirely on chance, or the uſe it is put to.

In its growth, three parts appear to be formed; one from the riſing core, which is the center, a ſecond on the outſide, and a third being the intermediate ſubſtance. Theſe appear to have three ſtages of duration; for that which forms on the core, I believe, makes the hair, and that on the outſide makes principally the plate of whalebone; this, when got a certain length, breaks off, leaving the hair projecting, becoming at the termination very brittle; and the third, or intermediate ſubſtance, by the time it riſes as high as the edge of the ſkin of the jaw, decays and ſoftens away like the old cuticle of the ſole of the foot when ſteeped in water.

The uſe of the whalebone, I ſhould believe, is principally for the retention of the food till ſwallowed; and do ſuppoſe the fiſh they catch are ſmall, when compared with the ſize of the mouth.

The œsophagus, as in other animals, begins at the fauces, or posterior part of the mouth; and, although circular at this part, is soon divided into two passages by the epiglottis passing across it, as will be described hereafter. Below its attachment to the trachea, it passes down in the posterior mediastinum, at some distance from the spine, to which it is attached by a broad part of the same membrane, and its anterior surface makes the posterior part of a cavity behind the pericardium.

Passing through the diaphragm it enters the stomach, and is lined with a very thick, soft, and white cuticle, which is continued into the first cavity of the stomach.

The inner, or true coat, is white, of a considerable density, and not muscular; but thrown into large longitudinal folds by the contraction of the muscular fibres of the œsophagus, which are very strong. It is very glandular; for on its inner surface, especially near the fauces, orifices of a vast number of glands are visible.

The œsophagus is larger in proportion to the bulk of the animal than in the quadruped, although not so much so as it usually is in fish, which we may suppose swallow their food much in the same way. In the Piked Whale it was three inches and an half wide.

The stomach, as in other animals, lies on the left side of the body, and terminates in the pylorus towards the right.

The duodenum passes down on the right side, very much as in the human subject, excepting that it is more exposed from the colon not crossing it. It lies on the right kidney, and then passes to the left side behind the ascending part of the colon and root of the mesentery, comes out on the left side, and getting on the edge of the mesentery becomes a loose intestine,

teſtine, forming the jejunum. In this courſe behind the meſentery it is expoſed, as in moſt quadrupeds, not being covered by it, as in the human. The jejunum and ilium paſs along the edge of the meſentery downwards to the lower part of the abdomen. The ilium near the lower end makes a turn towards the right ſide, and then mounting upwards, round the edge of the meſentery, paſſes a little way on the right, as high as the kidney, and there enters the colon, or cæcum. The cæcum lies on the lower end of the kidney, conſiderably higher than in the human body, which renders the aſcending part of the colon ſhort. The cæcum is about ſeven inches long, and more like that of the Lion or Seal than of any other animal I know.

The colon paſſes obliquely up the right ſide, a little towards the middle of the abdomen; and when as high as the ſtomach, croſſes to the left, and acquires a broad meſocolon: at this part it lies upon the left kidney, and in its paſſage down gets more and more to the middle line of the body. When it has reached the lower part of the abdomen, it paſſes behind the uterus, and along with the vagina, in the female; between the two teſticles, and behind the bladder and root of the penis, in the male, bending down to open on what is called the belly of the animal; and in its whole courſe it is gently convoluted. In thoſe which have no cæcum, and therefore can hardly be ſaid to have a colon, the inteſtine before its termination in the rectum makes the ſame kind of ſweep round the other inteſtines, as the colon does where there is a cæcum.

The inteſtines are not large for the ſize of the animal, not being larger in thoſe of eighteen or twenty-four feet long than in the Horſe, the colon not much more capacious than the jejunum and ilium, and very ſhort; a circumſtance common to carni-

vorous animals. In the Piked Whale, the length from the ſtomach to the cæcum is 28 yards and an half, length of cæcum ſeven inches, of the colon to the anus two yards and three quarters. The ſmall inteſtines are juſt five times the length of the animal, the colon with the cæcum a little more than one-half the length.

Thoſe parts that reſpect the nouriſhment of this tribe do not all ſo exactly correſpond as in land animals; for in theſe one in ſome degree leads to the other. Thus the teeth in the ruminating tribe point out the kind of ſtomach, cæcum, and colon; while in others, as the Horſe, Hare, Lion, &c. the appearances of the teeth only give us the kind of colon and cæcum; but in this tribe, whether teeth or no teeth, the ſtomachs do not vary much, nor does the circumſtance of cæcum ſeem to depend on either teeth or ſtomach. The circumſtances by which, from the form of one part we judge what others are, fail us here; but this may ariſe from not knowing all the circumſtances. The ſtomach, in all that I have examined, conſiſts of ſeveral bags, continued from the firſt on the left towards the right, where the laſt terminates in duodenum. The number is not the ſame in all; for in the Porpoiſe, Grampus, and Piked Whale, there are five; in the Bottle-noſe ſeven. Their ſize reſpecting one another differs very conſiderably; ſo that the largeſt in one ſpecies may in another be only the ſecond. The two firſt in the Porpoiſe, Bottle-noſe, and Piked Whale, are by much the largeſt; the others are ſmaller, although irregularly ſo.

The firſt ſtomach has, I believe, in all very much the ſhape of an egg, with the ſmall end downwards. It is lined every where with a continuation of the cuticle from the œſophagus. In the Porpoiſe the œſophagus enters the ſuperior end of the ſto-

mach. In the Piked Whale its entrance is a little way on the poſterior part of the upper end, and is oblique.

The ſecond ſtomach in the Piked Whale is very large, and rather longer than the firſt. It is of the ſhape of the Italic *S*, paſſing out from the upper end of the firſt on its right ſide, by nearly as large a beginning as the body of the bag. In the Porpoiſe it by no means bears the ſame proportion to the firſt, and opens by a narrower orifice; then paſſing down along the right ſide of the firſt ſtomach, it bends a little outwards at the lower end, and terminates in the third. Where this ſecond ſtomach begins, the cuticle of the firſt ends. The whole of the inſide of this ſtomach is thrown into unequal rugæ, appearing like a large irregular honeycomb. In the Piked Whale the rugæ are longitudinal, and in many places very deep, ſome of them being united by croſs bands; and in the Porpoiſe the folds are very thick, maſſy, and indented into one another. This ſtomach opens into the third by a round contracted orifice, which does not ſeem to be valvular.

The third ſtomach is by much the ſmalleſt, and appears to be only a paſſage between the ſecond and fourth. It has no peculiar ſtructure on the inſide, but terminates in the fourth by nearly as large an opening as its beginning. In the Porpoiſe it is not above one, and in the Bottle-noſe about five inches long.

The fourth ſtomach is of a conſiderable ſize; but a good deal leſs than either the firſt or ſecond. In the Piked Whale it is not round, but ſeems flattened between the ſecond and fifth. In the Porpoiſe it is long, paſſing in a ſerpentine courſe almoſt like an inteſtine. The internal ſurface is regular, but villous, and opens on its right ſide into the fifth, by a round opening ſmaller than the entrance from the third.

The fifth ſtomach is in the Piked Whale round, and in the Porpoiſe oval; it is ſmall, and terminates in the pylorus, which has little of a valvular appearance. Its coats are thinner than thoſe of the fourth, having an even inner ſurface, which is commonly tinged with bile.

The Piked Whale and, I believe, the large Whalebone Whale, have a cæcum; but it is wanting in the Porpoiſe, Grampus, and Bottle-noſe Whale.

The ſtructure of the inner ſurface of the inteſtine is in ſome very ſingular, and different from that of the others.

The inner ſurface of the duodenum in the Piked Whale is thrown into longitudinal rugæ, or valves, which are at ſome diſtance from each other, and theſe receive lateral folds. The duodenum in the Bottle-noſe ſwells out into a large cavity, and might almoſt be reckoned an eighth ſtomach; but as the gall ducts enter it I ſhall call it duodenum.

The inner coat of the jejunum, and ilium, appears in irregular folds, which may vary according as the muſcular coat of the inteſtine acts: yet I do not believe, that their form depends intirely on that circumſtance, as they run longitudinally, and take a ſerpentine courſe when the gut is ſhortened by the contraction of the longitudinal muſcular fibres. The inteſtinal canal of the Porpoiſe has ſeveral longitudinal folds of the inner coat paſſing along it, through the whole of its length. In the Bottle-noſe the inner coat, through nearly the whole track of the inteſtine, is thrown into large cells, and theſe again ſubdivided into ſmaller; the axis of which cells is not perpendicular to a tranſverſe ſection of the inteſtine, but oblique, forming pouches with the mouths downwards, and acting almoſt like valves, when any thing is attempted to be paſſed in a contrary direction: they begin faintly in the duodenum, before it makes

its quick turn, and terminate near the anus. The colon and rectum have the rugæ very flat, which seems to depend intirely on the contraction of the gut.

The rectum near the anus appears, for four or five inches, much contracted, is glandular, covered by a ſoft cuticle, and the anus ſmall.

I never found any air in the inteſtines of this tribe; nor indeed in any of the aquatic animals.

The meſenteric artery anaſtomoſes by large branches.

There is a conſiderable degree of uniformity in the liver of this tribe of animals. In ſhape it nearly reſembles the human, but is not ſo thick at the baſe, nor ſo ſharp at the lower edge, and is probably not ſo firm in its texture. The right lobe is the largeſt and thickeſt, its falciform ligament broad, and there is a large fiſſure between the two lobes, in which the round ligament paſſes. The liver towards the left is very much attached to the ſtomach, the little epiploon being a thick ſubſtance. There is no gall-bladder; the hepatic duct is large, and enters the duodenum about ſeven inches beyond the pylorus.

The pancreas is a very long, flat body, having its left end attached to the right ſide of the firſt cavity of the ſtomach: it paſſes acroſs the ſpine at the root of the meſentery, and near to the pylorus joins the hollow curve of the duodenum, along which it is continued, and adheres to that inteſtine, its duct entering that of the liver near the termination in the gut.

Although this tribe cannot be ſaid to ruminate, yet in the number of ſtomachs they come neareſt to that order; but here I ſuſpect that the order of digeſtion is in ſome degree inverted. In both the ruminants, and this tribe, I think it muſt be allowed that the firſt ſtomach is a reſervoir. In the ruminants the preciſe uſe of the ſecond and third ſtomachs is perhaps not known;

but

but digeſtion is certainly carried on in the fourth; while in this tribe, I imagine, digeſtion is performed in the ſecond, and the uſe of the third and fourth is not exactly aſcertained.

The cæcum and colon do not aſſiſt in pointing out the nature of the food and mode of digeſtion in this tribe. The Porpoiſe which has teeth, and four cavities to the ſtomach, has no cæcum, ſimilar to ſome land animals, as the Bear, Badger, Racoon, Ferret, Polecat, &c.; neither has the Bottle-noſe a cæcum which has only two ſmall teeth in the lower jaw; and the Piked Whale, which has no teeth, has a cæcum, almoſt exactly like the Lion, which has teeth and a very different kind of ſtomach.

The food of the whole of this tribe, I believe, is fiſh; probably each may have a particular kind, of which it is fondeſt, yet does not refuſe a variety. In the ſtomach of the large Bottle-noſe, I found the beaks of ſome hundreds of Cuttle-fiſh. In the Grampus I found the tail of a Porpoiſe; ſo that they eat their own genus. In the ſtomach of the Piked Whale, I found the bones of different fiſh, but particularly thoſe of the Dog-fiſh. From the ſize of the œſophagus we may conclude, that they do not ſwallow fiſh ſo large in proportion to their ſize as many fiſh do, that we have reaſon to believe take their food in the ſame way: for fiſh often attempt to ſwallow what is larger than their ſtomachs can at one time contain, and part remains in the œſophagus till the reſt is digeſted.

The epiploon on the whole is a thin membrane; on the right ſide it is rather a thin net-work, though on the left it is a complete membrane, and near to the ſtomach of the ſame ſide becomes of a conſiderable thickneſs, eſpecially between the two firſt bags of the ſtomach. It has little or no fat, except what

what ſlightly covers the veſſels in particular parts. It is attached forwards, all along to the lower part of the different bags conſtituting the ſtomach, and on the right to the root of the meſentery, between the ſtomach and tranſverſe arch of the colon, firſt behind to the tranſverſe arch of the colon and root of the meſentery, then to the poſterior ſurface of the left or firſt bag of the ſtomach, behind the anterior attachment. In ſome of this tribe there is the uſual paſſage behind the veſſels going to the liver, common to all quadrupeds I am acquainted with; but in others, as the ſmall Bottle-noſe, there is no ſuch paſſage, which by the cavity behind the ſtomach in the epiploon of this animal becomes a circumſcribed cavity.

The ſpleen is involved in the epiploon, and is very ſmall for the ſize of the animal. There are in ſome, as the Porpoiſe, one or two ſmall ones, about the ſize of a nutmeg, often ſmaller, placed in the epiploon behind the other. Theſe are ſometimes met with likewiſe in the human body.

The kidnies in the whole of this tribe of animals are conglomerated, being made up of ſmaller parts, which are only connected by cellular membrane, blood-veſſels, and ducts, or infundibula; but not partially connected by continuity of ſubſtance, as in the human body, the Ox, &c.: every portion is of a conical figure, whoſe apex is placed towards the center of the kidney, the baſe making the external ſurface; and each is compoſed of a cortical and tubular ſubſtance, the tubular terminating in the apex, which apex makes the mamilla. Each mamilla has an infundibulum, which is long, and at its beginning wide, embracing the baſe of the mamilla, and becoming ſmaller. Theſe infundibula unite at laſt, and form the ureter. The whole kidney is an oblong flat body, broader and thicker at the upper end than the lower, and has the

appearance

appearance of being made up of different parts placed close together, almost like the pavement of a street.

The ureter comes out at the lower end, and passes along to the bladder, which it enters very near the urethra.

The bladder is oblong, and small for the size of the animal. In the female the urethra passes along to the external sulcus or vulva, and opens just under the clitoris, much as in the human subject.

Whether being inhabitants of the water makes such a construction of kidney necessary I cannot say; yet one must suppose it to have some connection with such situation, since we find it almost uniformly take place in animals inhabiting the water, whether wholly, as this tribe, or occasionally, as the Manatee, Seal, and White Bear: there is, however, the same structure in the Black Bear, which, I believe, never inhabits the water. This, perhaps, should be considered in another light, as nature keeping up to a certain uniformity in the structure of similar animals; for the Black Bear in construction of parts is, in every other respect as well as this, like the White Bear.

The capsulæ renales are small for the size of the animal, when compared to the human, as indeed they are in most animals. They are flat, and of an oval figure; the right lies on the lower and posterior part of the diaphragm somewhat higher than the kidney; the left is situated lower down, by the side of the aorta, between it and the left kidney. They are composed of two substances; the external having the direction of its fibres or parts towards the center; the internal seeming more uniform, and not having so much of the fibrous appearance.

The blood of animals of this order is, I believe, similar to that of quadrupeds; but I have an idea, that the red globules

are

are in larger proportion. I will not pretend to determine how far this may aſſiſt in keeping up the animal heat; but as theſe animals may be ſaid to live in a very cold climate or atmoſphere, and ſuch as readily carries off heat from the body, they may want ſome help of this kind.

It is certain that the quantity of blood in this tribe and in the Seal is comparatively larger than in the quadruped, and therefore probably amounts to more than that of any other known animal.

This tribe differs from fiſh in having the red blood carried to the extreme parts of the body, ſimilar to the quadruped.

The cavity of the thorax is compoſed of nearly the ſame parts as in the quadruped; but there appears to be ſome difference, and the varieties in the different genera are greater.

The general cavity is divided into two, as in the quadruped, by the heart and mediaſtinum.

The heart in this tribe, and in the Seal, is probably larger in proportion to their ſize than in the quadruped, as alſo the blood-veſſels, more eſpecially the veins.

The heart is incloſed in its pericardium, which is attached by a broad ſurface to the diaphragm, as in the human body. It is compoſed of four cavities *, two auricles, and two ventricles: it is more flat than in the quadruped, and adapted to the ſhape of the cheſt. The auricles have more faſciculæ, and theſe paſs more acroſs the cavity from ſide to ſide than in many other animals; beſides, being very muſcular, they are very elaſtic,

* As the circulation is a permanent part of the conſtitution reſpecting the claſs to which the animal belongs, and as the kind of heart correſponds with the circulation, theſe ſhould be conſidered in the claſſing of animals. Thus we have animals whoſe hearts have only one cavity, others with two, three, and four cavities.

for being ſtretched they contract again very conſiderably. There is nothing uncommon or particular in the ſtructure of the ventricles, in the valves of the ventricles, or in that of the arteries.

The general ſtructure of the arteries reſembles that of other animals; and where parts are nearly ſimilar, the diſtribution is likewiſe ſimilar. The aorta forms its uſual curve, and ſends off the carotid and ſubclavian arteries.

Animals of this tribe, as has been obſerved, have a greater proportion of blood than any other known, and there are many arteries apparently intended as reſervoirs, where a larger quantity of arterial blood ſeemed to be required in a part, and vaſcularity could not be the only object. Thus we find, that the intercoſtal arteries divide into a vaſt number of branches, which run in a ſerpentine courſe between the pleura, ribs, and their muſcles, making a thick ſubſtance ſomewhat ſimilar to that formed by the ſpermatick artery in the Bull. Thoſe veſſels, every where lining the ſides of the thorax, paſs in between the ribs near their articulation, and alſo behind the ligamentous attachment of the ribs, and anaſtomoſe with each other. The medulla ſpinalis is ſurrounded with a net-work of arteries in the ſame manner, more eſpecially where it comes out from the brain, where a thick ſubſtance is formed by their ramifications and convolutions; and theſe veſſels moſt probably anaſtomoſe with thoſe of the thorax.

The ſubclavian artery in the Piked Whale, before it paſſes over the firſt rib, ſends down into the cheſt arteries which aſſiſt in forming the plexus on the inſide of the ribs; I am not certain but the internal mammary arteries contribute to form the anterior part of this plexus. The motion of the blood in ſuch muſt be very ſlow; the uſe of which we do not readily ſee. The deſcending aorta ſends off the intercoſtals, which are very large, and give branches to this plexus; and when it has reached the abdomen, it ſends off, as in the quadruped, the different

branches to the viſcera, and the lumbar arteries, which are likewiſe very large for the ſupply of that vaſt maſs of muſcles which moves the tail.

In our examination of particular parts, the ſize of which is generally regulated by that of the whole animal, if we have only been accuſtomed to ſee them in thoſe which are ſmall or middle-ſized, we behold them with aſtoniſhment in animals ſo far exceeding the common bulk as the Whale. Thus the heart and aorta of the Spermaceti Whale appeared prodigious, being too large to be contained in a wide tub, the aorta meaſuring a foot in diameter. When we conſider theſe as applied to the circulation, and figure to ourſelves, that probably ten or fifteen gallons of blood are thrown out at one ſtroke, and moved with an immenſe velocity through a tube of a foot diameter, the whole idea fills the mind with wonder.

The veins, I believe, have nothing particular in their ſtructure, excepting in parts requiring a peculiarity, as in the folds of the ſkin on the breaſt in the Piked Whale, where their elaſticity was to be increaſed.

Of the Larynx.

The larynx in moſt animals living on land is a compound organ, adapted both for reſpiration, deglutition, and ſound, which laſt is produced in the actions of reſpiration; but in this tribe the larynx, I ſuppoſe, is only adapted to reſpiration, as we do know that they have any mode of producing ſound.

It is compoſed of os hyoides, thyroid, cricoid, and two arytenoid cartilages, with the epiglottis. It varies very much in ſtructure and ſize, when compared in animals of different genera. Theſe cartilages were much ſmaller in the Bottle-noſe of twenty-four feet long, than in the Piked Whale of ſeventeen feet, while the os hyoides was much larger.

In

In the Bottle-nose, the os hyoides is compoſed of three bones, beſides two whoſe ends are attached to it, being placed above the os hyoides, making five in all. In the Porpoiſe, Piked Whale, &c. it is but one bone, ſlightly bent, having a broad thin proceſs paſſing up, which is a little forked: it has no attachment to the head by means of other bones, as in many quadrupeds.

The thyroid cartilage in the Piked Whale is broad from ſide to ſide, but not from the upper to the lower part: it has two lateral proceſſes, which are long, and paſs down the outſide of the cricoid, near to its lower end, and are joined to it much as in the human ſubject. Theſe differ in ſhape in different animals of this tribe.

The cricoid cartilage is broad and flat, making the poſterior and lateral part of the larynx, and is much deeper behind, and laterally, than before. It is extremely thick and ſtrong, flattened on the poſterior ſurface, and hollowed from the upper edge to the lower. It terminates by a thick edge on the poſterior part above, but irregularly at the lower edge, in the cartilages of the larynx.

The two arytenoid cartilages are extremely projecting, and united to each other till near their ends; are articulated on the upper edge of the cricoid, but ſend down a proceſs, which paſſes on the inſide of the cricoid, being attached to a bag in the Piked Whale, which is formed below the thyroid and before the cricoid cartilages; they croſs the cavity of the larynx obliquely, making the paſſage, at the upper part, a groove between them: the cavity at this place ſwells out laterally, but is very narrow between the anterior and poſterior ſurfaces. The paſſage above between the arytenoid and thyroid cartilages is wide from ſide to ſide, and is continued down on the outſide of the proceſſes of

the arytenoid cartilage, as well as between them, ending below the thyroid, which is folliculated on its inner ſurface on the fore part of the cricoid cartilage.

The epiglottis makes a third part of the paſſage, and compleats the glottis by forming it into a canal, in ſeveral of this tribe; but in the Piked Whale it was not attached to the two arytenoid cartilages, but only in contact, or incloſing them at their baſe, ſo as to make them form a complete canal.

I could not obſerve any thing like a thyroid gland.

From the glottis and epiglottis being ſo connected as to make but one canal, and from the thyroid and cricoid cartilages being ſo flattened in ſome between the anterior and poſterior ſurface, the paſſage through theſe parts is very ſmall or contracted; but the trachea ſwells out again into a very conſiderable ſize. Its larger branches are in proportion to the trunk, and enter the lungs at the upper end along with the blood-veſſels.

Of the Lungs.

The lungs are two oblong bodies, one on each ſide of the cheſt, and are not divided into ſmaller lobes, as in the human ſubject. They are of conſiderable length, but not ſo deep between the fore and back part, as in the quadruped, from the heart being broad, flat, and of itſelf filling up the fore part of the cheſt. They paſs further down on the back part than in the quadruped, by which their ſize is increaſed, and riſe higher up in the cheſt than the entrance of the veſſels, coming to a point at the upper end. From the entrance of the veſſels they are connected downwards, along their whole inner edge, by a ſtrong attachment (in which there are in ſome lymphatic glands) to

the posterior mediastinum. The lungs are extremely elastic in their substance, even so much so as to squeeze out any air that may be thrown into them, and to become almost at once a solid mass, having a good deal the appearance, consistence, and feel of an ox's spleen. The branches of the bronchiæ which ramify into the lungs have not the cartilages flat, but rather rounded; a construction which admits of greater motion between each.

The pulmonary cells are smaller than in quadrupeds, which may make less air necessary, and they communicate with each other, which those of the quadruped do not; for by blowing into one branch of the trachea, not only the part to which it immediately goes, but the whole lungs are filled.

As the ribs in this tribe do not completely make the cavity of the thorax, the diaphragm has not the same attachments as in the quadruped, but is connected forwards to the abdominal muscles, which are very strong, being a mixture of muscular and tendinous fibres.

The position of the diaphragm is less transverse than in the quadruped, passing more obliquely backwards, and coming very low on the spine, and higher up before; which makes the chest longest in the direction of the animal at the back, and gives room for the lungs to be continued along the spine.

The parts immediately concerned in inspiration are extremely strong; the diaphragm remarkably so. The reason of this must at once appear; it necessarily requiring great force to expand in a dense medium like water, especially too when the vacuity is to be filled with one which is rarer, and is to water a species of vacuum; the pressure being much greater on the external surface than the counter-pressure from within. But expiration on the other hand must be much more easily performed;

formed: the natural elaſticity of the parts themſelves, with the preſſure of the water on the external ſurface of the body, being greater than the reſiſtance of the air from within, will both tend to produce expiration without any immediate action of muſcles.

The diaphragm, in theſe animals, appears to be the principal agent in inſpiration; and the cavity of the thorax not being entirely ſurrounded by bony parts, is of courſe leſs eaſily expanded, and the apparatus for its expanſion in all directions, as in the quadruped, does not exiſt here.

The Blow-hole, or Paſſage for the Air.

As the noſe in every animal that breathes air is a common paſſage for the air, and is alſo the organ of ſmelling; I ſhall deſcribe it in this tribe as inſtrumental to both theſe purpoſes.

There is a variety in ſome ſpecies of this animal which is, I believe, peculiar to this order; that is, the want of the ſenſe of ſmelling: none of thoſe which I have yet examined having that ſenſe, except the two kinds of Whalebone Whale: ſuch of courſe have neither the olfactory nerves, nor the organ; therefore, in them, the noſtrils are intended merely for reſpiration; but others have the organ placed in this paſſage as in other animals.

The membranous portion of the poſterior noſtrils is one canal; but when in the bony part, in moſt of them, it is divided into two; the Spermaceti Whale, however, is an exception. In thoſe which have it divided, it is in ſome continued double through the anterior ſoft parts, opening by two orifices, as in the Piked Whale; but in others, it unites again in the membranous part, making externally only one orifice, as in the

Porpoise, Grampus, and Bottle-nose. At its beginning in the fauces, it is a roundish hole, surrounded by a strong sphincter muscle, for grasping the epiglottis; beyond this, the canal becomes larger, and opens into the two passages in the bones of the head. This part is very glandular, being full of follicles, whose ducts ramify in the surrounding substance, which appears fatty and muscular like the root of the tongue, and these ramifications communicate with one another, and contain a viscid slime.

In the Spermaceti Whale, which has a single canal, it is thrown a little to the left side. After these canals emerge from the bones near the external opening, they become irregular, and have several sulci passing out laterally, of irregular forms, with corresponding eminences. The structure of these eminences is muscular and fatty, but less muscular than the tongue of a quadruped.

In the Porpoise there are two sulci on each side; two large and two small, with corresponding eminences of different shapes, the large ones being thrown into folds. The Spermaceti Whale has the least of this structure; the external opening in it comes farther forwards towards the anterior part of the head, and is consequently longer than in others of this order. Near to its opening externally, it forms a large sulcus, and on each side of this canal is a cartilage, which runs nearly its whole length. In all that I have examined, this canal, forwards from the bones, is intirely lined with a thick cuticle of a dark colour.

In those which have only one external opening, it is transverse, as in the Porpoise, Grampus, Bottle-nose and Spermaceti Whale, &c.; where double, they are longitudinal, as in the Piked Whale, and the large Whalebone Whale. These openings

openings form a paſſage for the air in reſpiration to and from the lungs; for it would be impoſſible for theſe animals to breathe air through the mouth; indeed, I believe, the human ſpecies alone breathe by the mouth, and in them it is moſtly from habit; for in quadrupeds the epiglottis conducts the air into the noſe.

In the whole of this tribe, the ſituation of the opening on the upper ſurface of the head is well adapted for this purpoſe, being the firſt part that comes to the ſurface of the water in the natural progreſſive motion of the animal; therefore it is to be conſidered principally as a reſpiratory organ, and where it contains the organ of ſmell, that is only ſecondary.

As the animals of this order do not live in the medium which they inſpire, the organs conducting the air to the lungs are in ſome ſort particularly conſtructed, that the water in which they live may not interfere with the air they breathe.

The projecting glottis, which has been deſcribed, paſſes into the poſterior noſtrils, by which means it croſſes the fauces, dividing them into two paſſages. The enlargement at the termination of the glottis, obſerved in ſome of them, would ſeem to be intended to prevent its retraction; but, as it ſeems confined to the Porpoiſe and Grampus, it may, perhaps, in them anſwer ſome other purpoſe.

The beginning of the poſterior noſtrils, which anſwers to the palatum molle in the quadruped, having a ſphincter, the glottis is graſped by it, which renders its ſituation ſtill more ſecure, and the paſſages through the head, acroſs the fauces and along the trachea, are rendered one continued canal; this union of glottis and epiglottis with the poſterior noſtril, making only a kind of joint, admits of motion, and of dilatation and contraction of the fauces, in deglutition, from the epiglottis moving more in or out of the poſterior noſtril.

This

This conſtruction of parts anſwers a purpoſe ſimilar to that of the epiglottis in the quadruped; it may be conſidered as the epiglottis and the arytenoid cartilages joining, to make a tubular or cylindrical epiglottis, inſtead of a valvular one.

The reaſons why there ſhould be ſo peculiar a conſtruction of parts do not at firſt appear; but we certainly ſee by it an abſolute guard placed upon the lungs, that no water ſhould get into them.

This tribe being without the projecting tongue of the quadruped, and wanting its extenſive motion, and the power of ſucking things into the mouth, may probably require the conſtruction between the air and lungs to be more perfect; but how far it is ſo, I will not pretend to ſay.

The ſize of the Brain differs much in different genera of this tribe, and likewiſe in the proportion it bears to the bulk of the animal. In the Porpoiſe, I believe, it is largeſt, and perhaps in that reſpect comes neareſt to the human.

The ſize of the cerebellum in proportion to that of the cerebrum is ſmaller in the human ſubject than in any animal with which I am acquainted. In many quadrupeds, as the Horſe, Cow, &c. the diſproportion in ſize between cerebellum and cerebrum is not great, and in this tribe it is ſtill leſs; yet not ſo ſmall as in the bird, &c.

The whole brain in this tribe is compact, the anterior part of the cerebrum not projecting ſo far forwards as in either the quadruped or in the human ſubject; neither is the medulla oblongata ſo prominent, but flat, lying in a kind of hollow made by the two lobes of the cerebellum.

The brain is compoſed of cortical and medullary ſubſtances, very diſtinctly marked; the cortical being, in colour, like the tubular ſubſtance of a kidney; the medullary, very white.

Theſe ſubſtances are nearly in the ſame proportion as in the human brain. The two lateral ventricles are large, and in thoſe that have olfactory nerves are not continued into them as in many quadrupeds; nor do they wind ſo much outwards as in the human ſubject, but paſs cloſe round the poſterior ends of the thalami nervorum opticorum. The thalami themſelves are large; the corpora ſtriata ſmall; the crura of the fornix are continued along the windings of the ventricles, much as in the human ſubject. The plexus choroides is attached to a ſtrong membrane, which covers the thalami nervorum opticorum, and paſſes through the whole courſe of the ventricle, much as in the human ſubject.

The ſubſtance of the brain is more viſibly fibrous than I ever ſaw it in any other animal, the fibres paſſing from the ventricles as from a center to the circumference, which fibrous texture is alſo continued through the cortical ſubſtance. The whole brain in the Piked Whale weighed four pounds ten ounces.

The nerves going out from the brain, I believe, are ſimilar to thoſe of the quadruped, except in the want of the olfactory nerves in the genus of the Porpoiſe.

The medulla ſpinalis is much ſmaller in proportion to the ſize of the body than in the human ſpecies, but ſtill bears ſome proportion to the quantity of brain; for in the Porpoiſe, where the brain is largeſt, the medulla ſpinalis is largeſt; yet this did not hold good in the Spermaceti Whale, the ſize of the medulla ſpinalis appearing to be proportionally larger than the brain, which was ſmall when compared to the ſize of the animal. It has a cortical part in the center, and terminates about the twenty-fifth vertebra, beyond which is the cauda equina, the dura mater going no lower. The nerves which go

off from the medulla ſpinalis are more uniform in ſize than in the quadruped, there being no ſuch inequality of parts, nor any extremities to be ſupplied, except the fins.

The medulla ſpinalis is more fibrous in its ſtructure than in other animals; and when an attempt is made to break it longitudinally, it tears with a fibrous appearance, but tranſverſely it breaks irregularly.

The dura mater lines the ſkull, and forms in ſome the three proceſſes anſwerable to the diviſions of the brain, as in the human ſubject; but in others, this is bone. Where it covers the medulla ſpinalis, it differs from all the quadrupeds I am acquainted with, incloſing the medulla cloſely, and the nerves immediately paſſing out through it at the lower part, as they do at the upper, ſo that the cauda equina, as it forms, is on the outſide of the dura mater.

As the Organs of Senſe are variouſly formed in different animals, fitted for the various modes of impreſſion; and as the modes are either increaſed or varied, according to circumſtances which make no part of the ſenſe itſelf, but which are neceſſary for the œconomy of the animal, we find the ſenſes in this tribe varied in their conſtruction, and in ſome a ſenſe is even wholly wanting.

The organs of ſenſe, which appear to be adapted to every mode of life, are thoſe of touch and taſte; but thoſe of ſmell, ſight, and hearing, probably require to be varied according to circumſtances. Thus ſmell may be increaſed by a mode of impregnation, hearing by the vibration of different mediums, and ſight by the different powers of refraction of different mediums; therefore, as animals are intended by nature to be differently circumſtanced, ſo are the ſenſes formed.

Of the Senſe of Touch.

The cutis in this tribe appears, in general, particularly well calculated for ſenſation; the whole ſurface being covered with villi, which are ſo many veſſels, and we muſt ſuppoſe, nerves. Whether this ſtructure is only neceſſary for acute ſenſation, or whether it is neceſſary for common ſenſation, where the cuticle is thick, and conſiſting of many layers, I do not know. We may obſerve, that where it is neceſſary the ſenſe of touch ſhould be accurate, the villi are uſually thick and long, which probably is neceſſary, becauſe in moſt parts of the body, where the more acute ſenſations of touch are required, ſuch parts are covered by a thick cuticle. Of this the ends of our fingers, toes, and the foot of the hoofed animals, are remarkable examples.

Whether this ſenſe is more acute in water, I am not certain, but ſhould imagine it is.

Of the Senſe of Taſte.

The tongue, which is the organ of taſte, is alſo endowed with the ſenſe of touch. It is likewiſe to be conſidered, in the greateſt number of animals, as an inſtrument for mechanical purpoſes; but probably leſs ſo in this tribe than any other. However, even in theſe, it muſt have been formed with this view, ſince, merely as an organ of taſte, it would only have required ſurface, yet is a projecting body endowed with motion. In ſome, it is better adapted for motion than in others; and I ſhould ſuppoſe this to be requiſite, on account of the difference in the mode of catching the food, and in the act of ſwallowing. It is moſt projecting in thoſe with teeth, probably for the better

conducting

conducting the food, ſtep by ſtep, to the œſophagus; whereas, it does not ſeem ſo neceſſary to have ſuch management of the tongue in thoſe which have no teeth, and catch their food by merely opening the mouth, and ſwimming upon it, or by having their prey carried in by the water. In the Porpoiſe and Grampus it is firm in texture, compoſed of muſcle and fat, being pointed and ſerrated on its edges, like that of the Hog.

In the Spermaceti Whale the tongue was almoſt like a feather-bed. In the Piked Whale it was but gently raiſed, hardly having any lateral edges, and its tip projecting but little, yet, like every other tongue, compoſed of muſcle and fat. The extent between the two jaw bones in this Whale was very conſiderable, taking in the whole width of the head or upper jaw, and of courſe including the whalebone. This extent of ſurface, between jaw and jaw, having but little projection of tongue, is almoſt flat from ſide to ſide, is extremely elaſtic when contracted, and throws the inner membrane into a vaſt number of very ſmall folds, that run parallel to one another, but which are again thrown into a cloſe ſerpentine courſe by the elaſticity of the part in a contrary direction. From the tongue being capable of but little motion, there is only a ſmall maſs of muſcle required; and from the thinneſs of the jaw bones, the diſtance between the lower ſurface of the mouth and external ſurface of the ſkin is but ſmall; and this ſkin being ribbed, and very elaſtic, is capable of conſiderable diſtention, by which the cavity of the mouth can be enlarged.

The tongue of the large Whalebone Whale, I ſhould ſuppoſe, roſe in the mouth conſiderably; the two jaws at the middle being kept at ſuch a diſtance on account of the whalebone, ſo that the ſpace between, when the mouth is ſhut, muſt be filled up by the tongue.

Of the Senſe of Smelling.

In this tribe of animals there is ſomething very remarkable in what relates to the ſenſe of ſmelling; nor have I been able to diſcover the particular mode by which it is performed.

When we conſider theſe animals as quadrupeds, and only conſtructed differently in external form for progreſſive motion through water, we muſt ſee that it was neceſſary that all the ſenſes ſhould correſpond with this medium: we muſt therefore be at a loſs to conceive how they ſmell, ſince we may obſerve, that the organ for ſmelling water, as in fiſh, is very different from that formed to ſmell air; and as we muſt ſuppoſe this tribe are only to ſmell water, being the medium in which ſuch odoriferous particles can be diffuſed, we ſhould expect their organ to be ſimilar to that of fiſh; but in that caſe nature would have been obliged to have attached the noſe of a fiſh to an animal conſtructed like a quadruped; and it is contrary to the laws which are eſtabliſhed in the animal creation to mix parts of different animals together.

In many of this tribe there is no organ of ſmell at all; and in thoſe which have ſuch an organ, it is not that of a fiſh, therefore probably not calculated to ſmell water. It becomes difficult, therefore, to account for the manner in which ſuch animals ſmell the water; and why the others ſhould not have had ſuch an organ *,

* Is the mode of ſmelling in fiſh ſimilar to taſting in other animals? Or is the air contained in the water impregnated with the odoriferous parts, and this air the fiſh ſmells? If ſo, it is ſomewhat ſimilar to the breathing of fiſh, it not being the water which produces the effect there, but the air contained in it. This I proved by experiments, and is mentioned by Dr. PRIESTLEY.

which,

which, I believe, is peculiar to the large and ſmall Whale-bone Whales.

Although it is not the external air which they inſpire that produces ſmell, I believe it is the air retained in the noſtril out of the current of reſpiration, which by being impregnated with the odoriferous particles contained in the water during the act of blowing, is applied to the organ of ſmell. It might be ſuppoſed, that they could ſmell the air on the ſurface of the water by every inſpiration, as animals do on land; and probably they may: but this will not give them the power to ſmell the odoriferous particles of their prey in the water at any depth; and as their organ is not fitted to be affected by the application of water, and as they cannot ſuck water into the noſtril, without the danger of its paſſing into the lungs, it cannot be by its application to this organ that they are enabled to ſmell.

Some have the power of throwing the water from the mouth through the noſtril, and with ſuch force as to raiſe it thirty feet high: this muſt anſwer ſome important purpoſe, although not immediately evident to us.

As the organ appears to be formed to ſmell air only, and as I conceive the ſmelling of the external air could not be of uſe as a ſenſe. I therefore believe, that they do not ſmell in inſpiration; yet let us conſider how they may be ſuppoſed to ſmell the odoriferous particles of the water.

The organ of ſmell is out of the direct road of the current of air in inſpiration; it is alſo out of the current of water when they ſpout; may we not ſuppoſe then, that this ſinus contains air, and as the water paſſes in the act of throwing it out, that it impregnates this reſervoir of air, which immediately affects the ſenſe of ſmell. This operation is probably performed in the time of expiration, becauſe it is ſaid that this

water

water is ſometimes very offenſive; but all this I only give as conjecture.

If the above ſolution is juſt, then only thoſe which have the organ of ſmell can ſpout, a fact worthy of enquiry.

The organ of ſmell would appear to be leſs neceſſary in theſe animals than in thoſe which live in air, ſince ſome are wholly deprived of it; and the organ in thoſe which have it is extremely ſmall, when compared with that of other animals, as well as the nerve which is to receive the impreſſion, as was obſerved above.

Of the Senſe of Hearing.

The ear is conſtructed much upon the ſame principle as in the quadruped; but as it differs in ſeveral reſpects, which it is neceſſary to particulariſe, to convey a perfect idea of it the whole ſhould be deſcribed. As this would exceed the limits of this Paper, I ſhall content myſelf with a general deſcription, taking notice of thoſe material points in which it differs from that of the quadruped.

This organ conſiſts of the ſame parts as in the quadruped; an external opening, with a membrana tympani, an Euſtachian tube, a tympanum with its proceſſes, and the ſmall bones. There is no external projection forming a funnel, but merely an external opening. We can eaſily aſſign a reaſon why there ſhould be no projecting ear, as it would interfere with progreſſive motion; but the reaſon why it is not formed as in birds, is not ſo evident; whether the percuſſions of water could be collected into one point as air, I cannot ſay. The tympanum is conſtructed with irregularities, ſo much like thoſe of an external ear, that I could ſuppoſe it to have a ſimilar effect.

The

The external opening begins by a ſmall hole, ſcarcely perceptible, ſituated on the ſide of the head a little behind the eye. It is much longer than in other animals, in conſequence of the ſize of the head being ſo much increaſed beyond the cavity that contains the brain. It paſſes in a ſerpentine courſe, at firſt horizontally, then downwards, and afterwards horizontally again, to the membrana tympani, where it terminates. In its whole length it is compoſed of different cartilages, which are irregular and united together by cellular membrane, ſo as to admit of motion, and probably of lengthening or ſhortening, as the animal is more or leſs fat.

The bony part of the organ is not ſo much incloſed in the bones of the ſkull as in the quadruped, conſiſting commonly of a diſtinct bone or bones, cloſely attached to the ſkull, but in general readily to be ſeparated from it; yet in ſome it ſends off, from the poſterior part, proceſſes which unite with the ſkull. It varies in its ſhape, and is compoſed of the immediate organ and the tympanum.

The immediate organ is, in point of ſituation to that of the tympanum, ſuperior and internal, as in the quadruped. The tympanum is open at the anterior end, where the Euſtachian tube begins.

The Euſtachian tube opens on the outſide of the upper part of the fauces: in ſome higher in the noſe than others; higheſt, I believe, in the Porpoiſe. From the cavity of the tympanum, where it is rather largeſt, it paſſes forwards and inwards, and near its termination appears very much faſciculated, as if glandular.

The Euſtachian tube and tympanum communicate with ſeveral ſinuſes, which paſſing in various directions ſurround the bone of the ear. Some of theſe are cellular, ſimilar to the cells of the maſtoid proceſs in the human ſubject, although not bony. There is a portion of this cellular ſtructure of a

particular kind, being white, ligamentous, and each part rather rounded, than having flat ſides*. One of the ſinuſes paſſing out of the tympanum cloſe to the membrana tympani, goes a little way in the ſame direction, and communicates with a number of cells.

The whole function of the Euſtachian tube is perhaps not known; but it is evidently a duct from the cavity of the ear, or a paſſage for the mucus of theſe parts; the external opening having a particular form would incline us to believe, that ſomething was conveyed to the tympanum.

The bony part of the organ is very hard and brittle, rendering it even difficult to be cut with a ſaw, without its chipping into pieces. That part which contains the immediate organ is by much the hardeſt, and has a very ſmall portion of animal ſubſtance in it; for when ſteeped in an acid, what remains is very ſoft, almoſt like a jelly, and laminated. The bone is not only harder in its ſubſtance, but there is on the whole more ſolid bone than in the correſponding parts of quadrupeds, it being thick and maſſy.

The part containing the tympanum is a thin bone, coiled upon itſelf, attached by one end to the portion which contains the organ; and this attachment in ſome is by cloſe contact only, as in the Narwhale; in others, the bones run into one another, as in the Bottle-noſe and Piked Whales.

The concave ſide of the tympanum is turned towards the organ, its two edges being cloſe to it; the outer is irregular, and in many only in contact, as in the Porpoiſe: while in

* Theſe communications with the Euſtachian tube may be compared to a large bag on the baſes of the ſkull of the Horſe and Aſs, which is a lateral ſwell of the membranous part of the tube, and when diſtended will contain nearly a quart.

others

others the union is by bony continuity, as in the Bottle-nose Whale, leaving a passage on which the membrana tympani is stretched, and another opening, which is the communication with the sinuses.

The surface of the bone containing the immediate organ opposite to the mouth of the tympanum is very irregular, having a number of eminences and cavities. The cavity of the tympanum is lined with a membrane, which also covers the small bones with their muscles, and appears to have a thin cuticle. This membrane renders the bones, muscles, tendons, &c. very obscure, which are seen distinctly when that is removed. It appears to be a continuation of the periosteum, and the only uniting substance between the small bones. Besides the general lining, there is a plexus of vessels, which is thin and rather broad, and attached by one edge, the rest being loose in the cavity of the tympanum, somewhat like the plexus choroides in the ventricles of the brain. The cavity, we may suppose, intended to increase sound, probably by the vibration of the bone; and from its particular formation we can easily conceive, that the vibrations are conducted, or reflected, towards the immediate organ, it being in some degree a substitute for the external ear.

The external opening being smaller than in any animals of the same size, the membrana tympani is nearly in the same proportion. In the Bottle-nose Whale, the Grampus, and Porpoise, it is smooth and concave externally, but of a particular construction on the inner surface; for a tendinous process passes from it towards the malleus, converging as it proceeds from the membrane, and becoming thinner till its insertion into that bone. I could not discover whether it had any muscular fibres which could affect the action of the malleus. In

the Piked Whale, the termination of the external opening, inſtead of being ſmooth and concave, is projecting, and returns back into the meatus for above an inch in length, is firm in texture, with thick coats, is hollow on its inſide, and its mouth communicating with the tympanum; one ſide being fixed to the malleus, ſimilar to the tendinous proceſs which goes from the inſide of the membrana tympani in the others.

A little way within the membrana tympani, are placed the ſmall bones, which are three in number, as in the quadruped, Malleus, Incus, and Stapes; but in the Bottle-noſe Whale there is a fourth, placed on the tendon of the Stapedæus muſcle. Theſe bones are as it were ſuſpended between the bone of the tympanum, and that of the immediate organ.

The malleus has two attachments, beſides that with the incus; one cloſe to the bone of the tympanum, which, in the Porpoiſe, is only by contact, but in others by a bony union; the other attachment is formed by the tendon, above deſcribed, being united to the inner ſurface of the membrana tympani. Its baſe articulates with the incus.

The incus is attached by a ſmall proceſs to the tympanum, and is ſuſpended between the malleus and ſtapes. The proceſs by which it articulates with the ſtapes is bent towards that bone.

The ſtapes ſtands on the veſtibulum, by a broad oval baſe. In many of this tribe, the opening from ſide to ſide of the ſtapes is ſo ſmall as hardly to give the idea of a ſtirrup.

The muſcles which move theſe bones are two in number, and tolerably ſtrong. One ariſes from that projecting part of the tympanum which goes to form the Euſtachian tube, and running backwards is inſerted into a ſmall depreſſion on the anterior part of the malleus. The uſe of this muſcle ſeems to

be to tighten the membrana tympani; but in thoſe which have the malleus anchyloſed with the tympanum, we can hardly conjecture its uſe. The other has its origin from the inner ſurface of the tympanum, and paſſing backwards is inſerted into the ſtapes by a tendon, in which I found a bone in the large Bottle-noſe. This muſcle gives the ſtapes a lateral motion. What particular uſe in hearing may be produced by the action of theſe muſcles, I will not pretend to ſay; but we muſt ſuppoſe, whatever motion is given to the bones muſt terminate in the movement of the ſtapes.

The immediate organ of hearing is contained in a round, bony proceſs, and conſiſts of the Cochlea and Semicircular Canals, which ſomewhat reſemble the quadruped; but, beſides the two ſpiral turns of the cochlea, there is a third, which makes a ridge within that continued from the foramen rotundum, and follows the turns of the canal.

The cochlea is much larger, when compared with the ſemicircular canals, than in the human ſpecies and quadruped.

We may reckon two paſſages into the immediate organ of hearing, the foramen rotundum, and foramen ovale. They are at a greater diſtance than in the quadruped. The foramen rotundum is placed much more on the outer ſurface of the bone, and not in the cavity of the bony tympanum; but may be ſaid to communicate with the ſurrounding cellular part of the tympanum. The foramen rotundum, which is the beginning of one of theſe turns, appears to be only one end of a tranſverſe groove, which is afterwards cloſed in the middle, forming a canal with the two ends open; ſo that this foramen appears to have two beginnings; but the other opening is probably only a paſſage for blood-veſſels going to the cochlea.

From

From this foramen begins the inner turn of the cochlea, which is the largeſt, eſpecially at its beginning; the other begins from the veſtibulum. The cochlea is a ſpiral canal coiled within itſelf, and divided into two by a thin ſpiral bony plate, which is compleated in the recent ſubject, and forms two perfect canals.

In the recent ſubject, the foramen rotundum is lined with the membrane of the tympanum, which terminates in a blind end, forming a kind of membrana cochleæ. The other opening, in the recent ſubject, communicates with the ſpiral turn, beyond the membranous termination of the foramen rotundum.

The foramen ovale has a little projection inwards all round, on which the ſtapes ſtands: within this is the veſtibulum, which is common to the other ſpiral turn of the cochleæ, and the ſemicircular canals; this canal of the cochlea paſſes out firſt in a direction contrary to its general courſe, but ſoon makes a turn into the ſpiral. It is round, and not merely a diviſion of the cochlea into two by a ſeptum, but has a membrane of its own, which is attached to the thin bony plate, and lines that part of the cochlea in ſuch a manner as to retain its ſtructure when the bone is removed. The cochlea in ſome compleats one turn and an half; in others, more. It is not a ſpiral on a plane, or cylinder, but on a cone.

I have already obſerved, that by looking in at the foramen rotundum, we ſee two ſmall ridges; the uppermoſt is the ſwell of the canal from the veſtibulum juſt deſcribed; the lower ridge, which is alſo a canal, may be obſerved juſt to paſs along the foramen belonging to this canal, cloſe to the ſeptum between the two; a circumſtance, I believe, peculiar to this tribe. Its beginning is cloſe to the veſtibulum, but does not open from it, and paſſes along the firſt deſcribed ſpiral

turn to its apex: when opened, it appears to be a canal full of ſmall perforations, probably the paſſages of the branches from the auditory nerve.

This bony proceſs has ſeveral perforations in it; one of them large, for the paſſage of the ſeventh pair of nerves. The ſize of the portio mollis, before its entrance into the organ, is very large, and bears no proportion to that which enters. The paſſage for this nerve is very wide, and ſeems to have an irregular blind conical, and ſomewhat ſpiral, termination; its being ſpiral ariſes from the cloſeneſs to the point of the cochlea.

In the terminating part there are a number of perforations into the cochlea, and one into the ſemicircular canals, which afford a paſſage to the different diviſions of the auditory nerve. There is a conſiderable foramen in its anterior ſide near the bottom, for the paſſage of the portio dura, and which is continued backward to the cavity of the tympanum near the ſtapes, and emerges near the poſterior and upper part of this bone.

Of the Organ of Seeing.

The eye in this tribe of animals is conſtructed upon nearly the ſame principle as that of quadrupeds, differing, however, in ſome circumſtances; by which it is probably better adapted to ſee in the medium through which the light is to paſs. It is upon the whole ſmall for the ſize of the animal, which would lead to the ſuppoſition, that their locomotion is not great; for, I believe, animals that ſwim are in this reſpect ſimilar to thoſe that fly; and as this tribe come to the ſurface of the medium in which they live, they may be conſidered in the ſame view with birds which ſoar; and we find, birds

that

that fly to great heights, and move through a conſiderable ſpace, in ſearch of food, have their eyes larger in proportion to their ſize.

The eyelids have but little motion, and do not conſiſt of looſe cellular membrane, as in quadrupeds, but rather of the common adipoſe membrane of the body; the connexion, however, of their circumference with the common integuments is looſe, the cellular membrane being leſs loaded with oil, which allows of a ſlight fold being made upon the ſurrounding parts in opening the eyelids. This is not to an equal degree in them all, being leſs ſo in the Porpoiſe than in the Piked Whale.

The tunica conjunctiva, where it is reflected from the eyelid to the eyeball, is perforated all round by ſmall orifices of the ducts of a circle of glandular bodies lying behind it.

The lachrymal gland is ſmall; its uſe being ſupplied by thoſe above-mentioned; and the ſecretion from them all, I believe, to be a mucus ſimilar to what is found in the Turtle and Crocodile. There are neither puncta nor lachrymal duct, ſo that the ſecretion, whatever it be, is waſhed off into the water.

The muſcles which open the eyelids are very ſtrong: they take their origin from the head, round the optic nerve, which in ſome requires their being very long, and are ſo broad as almoſt to make one circular muſcle round the whole of the interior ſtraight muſcles of the eye itſelf. They may be divided into four; a ſuperior, an inferior, and one at each angle: as they paſs outwards to the eyelids, they diverge and become broader, and are inſerted into the inſide of the eyelids almoſt equally all round. They may be termed the dilatores of the eyelids; and, before they reach their inſertion, give off the external ſtraight muſcles, which are ſmall, and inſerted into the ſclerotic coat before the tranſverſe axis of the eye: theſe

theſe may be named the elevator, depreſſor, adductor, and abductor, and may be diſſected away from the others as diſtinct muſcles. Beſides theſe four going from the muſcles of the eyelid to the eye itſelf, there are two which are larger, and incloſe the optic nerve with the plexus. As theſe paſs outwards they become broad, may in ſome be divided into four, and are inſerted into the ſclerotic coat, almoſt all round the eye, rather behind its tranſverſe axis.

The two oblique muſcles are very long; they paſs through the muſcles of the eyelids, are continued on to the globe of the eye, between the two ſets of ſtraight muſcles, and at their inſertions are very broad; a circumſtance which gives great variation to the motion of the eye.

The ſclerotic coat gives ſhape to the eye, both externally and internally, as in other animals; but the external ſhape and that of the internal cavity are very diſſimilar, ariſing from the great difference in the thickneſs of this coat in different parts. The external figure is round, except that it is a little flattened forwards; but that of the cavity is far otherwiſe, being made up of ſections of various circles, being a little lengthened from the inner ſide to the outer, a tranſverſe ſection making a ſhort ellipſis.

In the Piked Whale the long axis is two inches and three-quarters, the ſhort axis two inches and one-eighth.

The poſterior part of the cavity is a tolerably regular curve, anſwering to the difference in the two axiſes; but forwards, near the cornea, the ſclerotic coat turns quickly in, to meet the cornea, which makes this part of the cavity extremely flat, and renders the diſtance between the anterior part of the ſcle-

rotic coat and the bottom of the eye not above an inch and a quarter.

In the Piked Whale the ſclerotic coat, at its poſterior part, is very thick: near the extreme of the ſhort axis it was half an inch, and at the long axis one-eighth of an inch thick. In the Bottle-noſe Whale, the extreme of the ſhort axis was half an inch thick, and the extremes of the long axis about a quarter of an inch, or half the other.

The ſclerotic coat becomes thinner as it approaches to its union with the cornea, where it is thin and ſoft. It is extremely firm in its texture, where thick, and from a tranſverſe ſection would ſeem to be compoſed of tendinous fibres, intermixed with ſomething like cartilage; in this ſection four paſſages for veſſels remain open. This firmneſs of texture precludes all effect of the ſtraight muſcles on the globe of the eye, by altering its ſhape, and adapting its focus to different diſtances of objects, as has been ſuppoſed to be the caſe in the human eye.

The cornea makes rather a longer ellipſis than the ball of the eye; the ſides of which are not equally curved, the upper being moſt conſiderably ſo. It is a ſegment of a circle ſomewhat ſmaller than that of the eyeball, is ſoft and very flaccid.

The tunica choroides reſembles that of the quadruped; and its inner ſurface is of a ſilver hue, without any nigrum pigmentum.

The nigrum pigmentum only covers the ciliary proceſſes, and lines the inſide of the iris.

The retina appears to be nearly ſimilar to that of the quadruped.

The arteries going to the coats of the eye form a plexus paſſing round the optic nerve, reſembling, in its appearance, that of the ſpermatic artery in the Bull and ſome other animals.

The

The cryſtalline humor reſembles that of the quadruped; but whether it is very convex or flattened, I cannot determine, thoſe I have examined having been kept too long to preſerve their exact ſhape and ſize. The vitreous humor adhered to the retina at the entrance of the optic nerve.

The optic nerve is very long in ſome ſpecies, owing to the vaſt width of the head.

I ſhall not at preſent conſider the eye in animals of this tribe, as it reſpects the power of viſion, that being performed on a general principle common to every animal inhabiting the water; more eſpecially as I am only maſter of the conſtruction and formation of the eye, and not of the ſize, ſhape, and denſities of the humors; yet, from reaſoning, we muſt ſuppoſe them to correſpond with the ſhape of the eye, and the medium through which the light is to paſs.

Of the Parts of Generation.

The parts of generation in both ſexes of this order of animals come nearer in form to thoſe of the ruminating than of any others; and this ſimilarity is, perhaps, more remarkable in the female than in the male; for their ſituation in the male muſt vary on account of external form, as was before obſerved.

The teſticles retain the ſituation in which they were formed, as in thoſe quadrupeds in which they never come down into the ſcrotum. They are ſituated near the lower part of the abdomen, one on each ſide, upon the two great depreſſors of the tail. At this part of the abdomen, the teſticles come in contact with the abdominal muſcles anteriorly.

The vaſa deferentia paſs directly from the epididymis behind the bladder, or between it and the rectum, into the urethra;

and there are no bags ſimilar to thoſe called veſiculæ ſeminales in certain other animals.

The ſtructure of the penis is nearly the ſame in them all, and formed much upon the principle of the quadruped. It is made up of two crura, uniting into one corpus cavernoſum, and the corpus ſpongioſum ſeems firſt to enter the corpus cavernoſum. In the Porpoiſe, at leaſt, the urethra is found nearly in the center of the corpus cavernoſum; but towards the glans ſeems to ſeparate or emerge from it, and becoming a diſtinct ſpongy body, runs along its under ſurface, as in quadrupeds. The corpus cavernoſum in ſome is broader from the upper part to the lower than from ſide to ſide; but in the Porpoiſe has the appearance of being round, becoming ſmaller forwards, ſo as to terminate almoſt in a point ſome diſtance from the end of the penis. The glans does not ſpread out as in many quadrupeds, but ſeems to be merely a plexus of veins covering the anterior end of the penis, yet is extended a good way further on, and is in ſome no more than one vein deep.

The crura penis are attached to two bones, which are nearly in the ſame ſituation and in the ſame part of the pelvis as thoſe to which the penis is attached in quadrupeds; but theſe bones are only for the inſertion of the crura, and not for the ſupport of any other part, like the pelvis in thoſe animals which have poſterior extremities, neither do they meet at the fore part, or join the vertebræ of the back.

The erectores penis are very ſtrong muſcles, having an origin and inſertion ſimilar to thoſe of the human ſubject.

The acceleratores muſcles are likewiſe very ſtrong; and there is a ſtrong and long muſcle, ariſing from the anus, and paſſing forwards to the bulb of the penis, that runs along the under ſurface of the urethra, and is at laſt loſt or inſerted in the cor-

pus ſpongioſum. This muſcle draws the penis into the prepuce, and throws that part of the penis that is behind its inſertion into a ſerpentine form. It is common to moſt animals that draw back the penis into what is called the ſheath, and may be called the retractor penis.

In all the females which I have examined, the parts of generation are very uniformly the ſame; conſiſting of the external opening, the vagina, the two horns of the uterus, Fallopian tubes, fimbriæ, and ovaria.

The external opening is a longitudinal ſlit, or oblong opening, whoſe edges meet in two oppoſite points, and the ſides are rounded off, ſo as to form a kind of ſulcus. The ſkin and parts on each ſide of this ſulcus are of a looſer texture than on the common ſurface of the animal, not being loaded with oil, and allowing of ſuch motion of one part on another as admits of dilatation and contraction. The vagina paſſes upwards and backwards towards the loins, ſo that its direction is diagonal reſpecting the cavity of the abdomen, and then divides into the two horns, one on each ſide of the loins; theſe afterwards terminating in the Fallopian tubes, to which the ovaria are attached. From each ovarium there is a ſmall fold of the peritoneum, which paſſes up towards the kidney of the ſame ſide, as in moſt quadrupeds.

The inſide of the vagina is ſmooth for about one-half of its length, and then begins to form ſomething ſimilar to valves projecting towards the mouth of the vagina, each like an os tincæ; theſe are about ſix, ſeven, eight, or nine in number. Where they begin to form, they hardly go quite round, but the laſt are complete circles. At this part too the vagina becomes ſmaller, and gradually decreaſes in width to its termination. From the laſt projecting part, the paſſage is continued

nued up to the opening of the two horns, and the inner surface of this laſt part is thrown into longitudinal rugæ, which are continued into the horns. Whether this laſt part is to be reckoned common uterus or vagina, and that the laſt valvular part is to be conſidered as os tincæ, I do not know; but from its having the longitudinal rugæ, I am inclined to think it is uterus, this ſtructure appearing to be intended for diſtinction.

The horns are an equal diviſion of this part; they make a gentle turn outwards, and are of conſiderable length. Their inner ſurface is thrown into longitudinal rugæ, without any ſmall protuberances for the cotyledons to form upon, as in thoſe of ruminating animals; and where they terminate, the Fallopian tubes begin.

In the Bottle-noſe Whale, where the Fallopian tubes opened into the horns of the uterus, they were ſurrounded by pendulous bodies hanging looſe in the horns.

The Fallopian tubes, at their termination in the uterus, are remarkably ſmall for ſome inches, and then begin to dilate rather ſuddenly; and the nearer to the mouth the more this dilatation increaſes, like the mouth of a French horn, the termination of which is five or ſix inches in diameter. They are very full of longitudinal rugæ through their whole length.

The ovaria are oblong bodies, about five inches in length; one end attached to the mouth of the Fallopian tube, and the other near to the horn of the uterus. They are irregular on their external ſurface, reſembling a capſula renalis or pancreas. They have no capſula, but what is formed by the long Fallopian tube.

How the male and female copulate, I do not know; but it is alledged, that their poſition in the water is erect at that time, which I can readily ſuppoſe may be true; for otherwiſe, if

if the connexion is long, it would interfere with the act of reſpiration, as in any other poſition the upper ſurface of the heads of both could not be at the ſurface of the water at the ſame time. However, as in the parts of generation they moſt reſemble thoſe of the ruminating kind, it is poſſible they may likewiſe reſemble them in the duration of the act of copulation; for, I believe, all the ruminants are quick in this act.

Of their uterine geſtation I as yet know nothing; but it is very probable, that they have only a ſingle young one at a time, there being only two nipples. This ſeemed to be the caſe with the Bottle-noſe Whale, caught near Berkeley, which had been ſeen for ſome days with one young one following it, and they were both caught together.

The glands for the ſecretion of milk are two; one on each ſide of the middle line of the belly at its lower part. The poſterior ends, from which go out the nipples, are on each ſide of the opening of the vagina, in ſmall ſulci. They are flat bodies lying between the external layer of fat and abdominal muſcles, and are of conſiderable length, but only one-fourth of that in breadth. They are thin, that they may not vary the external ſhape of the animal, and have a principal duct, running in the middle through the whole length of the gland, and collecting the ſmaller lateral ducts, which are made up of thoſe ſtill ſmaller. Some of theſe lateral branches enter the common trunk in the direction of the milk's paſſage, others in the contrary direction, eſpecially thoſe neareſt to the termination of the trunk in the nipple. The trunk is large, and appears to ſerve as a reſervoir for the milk, and terminates externally in a projection, which is the nipple. The lateral portions of the ſulcus which incloſes the nipple, are compoſed of parts looſer in texture than the common adipoſe membrane, which is probably to admit of the elongation or projection of

the nipple. On the outſide of this there is another ſmall fiſſure, which, I imagine, is likewiſe intended to give greater facility to the movements of all theſe parts.

The milk is probably very rich; for in that caught near Berkeley with its young one, the milk, which was taſted by Mr. JENNER and Mr. LUDLOW, Surgeon, at Sodbury, was rich like Cow's milk to which cream had been added.

The mode in which theſe animals muſt ſuck would appear to be very inconvenient for reſpiration, as either the mother or young one will be prevented from breathing at the time, their noſtrils being in oppoſite directions, therefore the noſe of one muſt be under water, and the time of ſucking can only be between each reſpiration. The act of ſucking muſt likewiſe be different from that of land animals; as in them it is performed by the lungs drawing the air from the mouth backwards into themſelves, which the fluid follows, by being forced into the mouth from the preſſure of the external air on its ſurface; but in this tribe, the lungs having no connexion with the mouth, ſucking muſt be performed by ſome action of the mouth itſelf, and by its having the power of expanſion.

EXPLA-

EXPLANATION OF THE PLATES.

PLATE XVI.

This fish is called a Grampus: it was caught in the mouth of the river Thames, in the year 1759, and brought up to Westminster Bridge in a barge. It was twenty-four feet long.

PLATE XVII.

Another species of Grampus, which was caught in the river Thames, fifteen years ago. It was eighteen feet long.

PLATE XVIII.

Fig. 1. A species of Bottle-nose Whale; the Delphinus Delphis of Linnæus. It was caught upon the sea-coast, near Berkeley, where it had been seen for several days, following its mother, and was taken along with the old one, and sent up to me whole, for examination, by Mr. Jenner, Surgeon, at Berkeley. The old one was eleven feet long.

Fig. 2. The head of the same Whale as fig. 1. to shew the shape of the blow-hole, which is transverse, and almost semicircular.

PLATE XIX.

The Bottle-nose Whale described by Dale. It is similar to that of Plate XVIII. in its general form, but has only two small pointed teeth in the fore part of the upper jaw, and is rather lighter coloured on the belly. It was caught above

London Bridge in the year 1783, and became the property of the late Mr. Alderman PUGH, who very politely allowed me to examine its ſtructure, and to take away the bones. It was twenty-one feet long.

PLATE XX.

Fig. 1. The Balæna Roſtrata of FABRICIUS, or Piked Whale. It was caught upon the Dogger Bank. It had met with ſome accident between the two lower jaws under the tongue, in which part a conſiderable collection of air had taken place, ſo as to raiſe up the tongue and its attachments into a round body in the mouth, projecting even beyond the jaws. This rendered the head ſpecifically lighter than the water, ſo that it could not ſink, and therefore was eaſily caught.

It was ſeventeen feet long, and was brought to St. George's Fields, where I purchaſed it. The dorſal fin having been cut off cloſe to the back, is therefore only marked by a dotted line.

Fig. 2. A view of the tail, to ſhew its breadth.

PLATE XXI.

Includes the external parts of generation, with the relative ſituation of the anus and the nipples, of the Balæna Roſtrata.

Fig. 1. The labia pudendi ſpread open, expoſing the meatus urinarius, vagina, and anus, which in a natural ſtate are all concealed, there only appearing a long ſlit, the two edges of which are in contact.

AA. The labia pudendi.

B. The clitoris.

C. The meatus urinarius.

D. The

D. The opening of the vagina.

E. The anus.

Fig. 2. The ſulcus, in which the left nipple lies, ſpread open, and the nipple itſelf expoſed to view.

Fig. 3. The ſulcus of the right nipple, in a natural ſtate, only appearing like a line.

Fig. 4. A ſulcus near to the nipple, which is ſpread open to ſhew the inſide. This ſulcus, I conceive, gives a freedom to the motion of the ſkin of theſe parts, ſo as to allow the nipple to be more freely expoſed.

Fig. 5. The ſame ſulcus on the oppoſite ſide, cloſed up.

PLATE XXII.

A ſide view of one of the plates of whalebone of the Balæna Roſtrata.

A. The part of the plate which projects beyond the gum.

B. The portion which is ſunk into the gum.

CC. A white ſubſtance, which ſurrounds the whalebone, forming there a projecting bead, and alſo paſſing between the plates, to form their external lamellæ.

DD. The part analogous to the gum.

E. A fleſhy ſubſtance, covering the jaw bone, and on which the inner lamella of the plate is formed.

F. The termination of the plate of whalebone in a kind of hair.

PLATE XXIII.

Fig. 1. A perpendicular ſection of ſeveral plates of whalebone in their natural ſituation in the gum; their inner edges, or ſhorteſt terminations, are removed, and the cut edges of the plates ſeen from the inſide of the mouth.

The upper part ſhews the rough ſurface formed by the hairy termination of each plate of whalebone.

The middle part ſhews the diſtance the plates of whalebone are from each other.

The lower part ſhews the white ſubſtance in which they grow, and alſo the baſis on which they ſtand.

Fig. 2. An outline conſiderably magnified, to ſhew the mode of growth of the plates, and of the white intermediate ſubſtance.

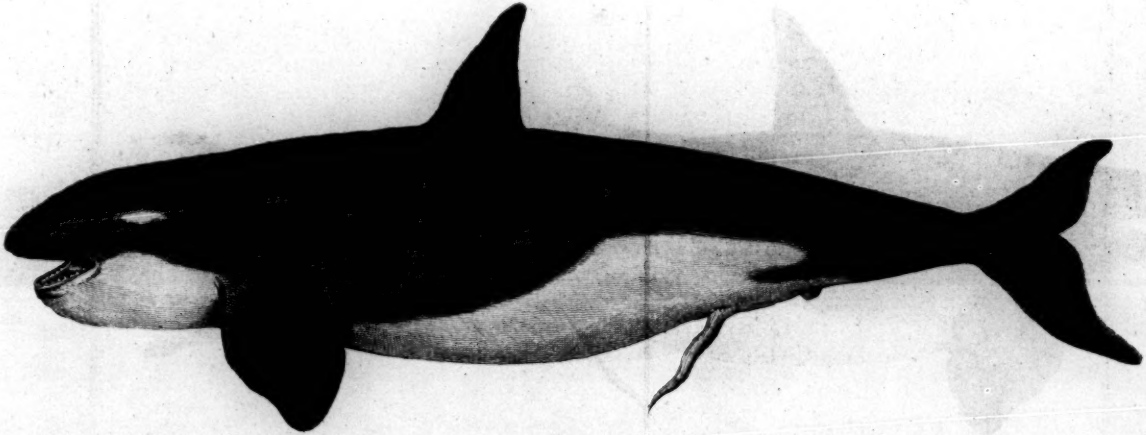
A. The middle layer of the plate, which is formed upon a pulp or cone that paſſes up in the centre of the plate. The termination of this layer forms the hair.

B. One of the outer layers, which grows, or is formed, from the intermediate white ſubſtance.

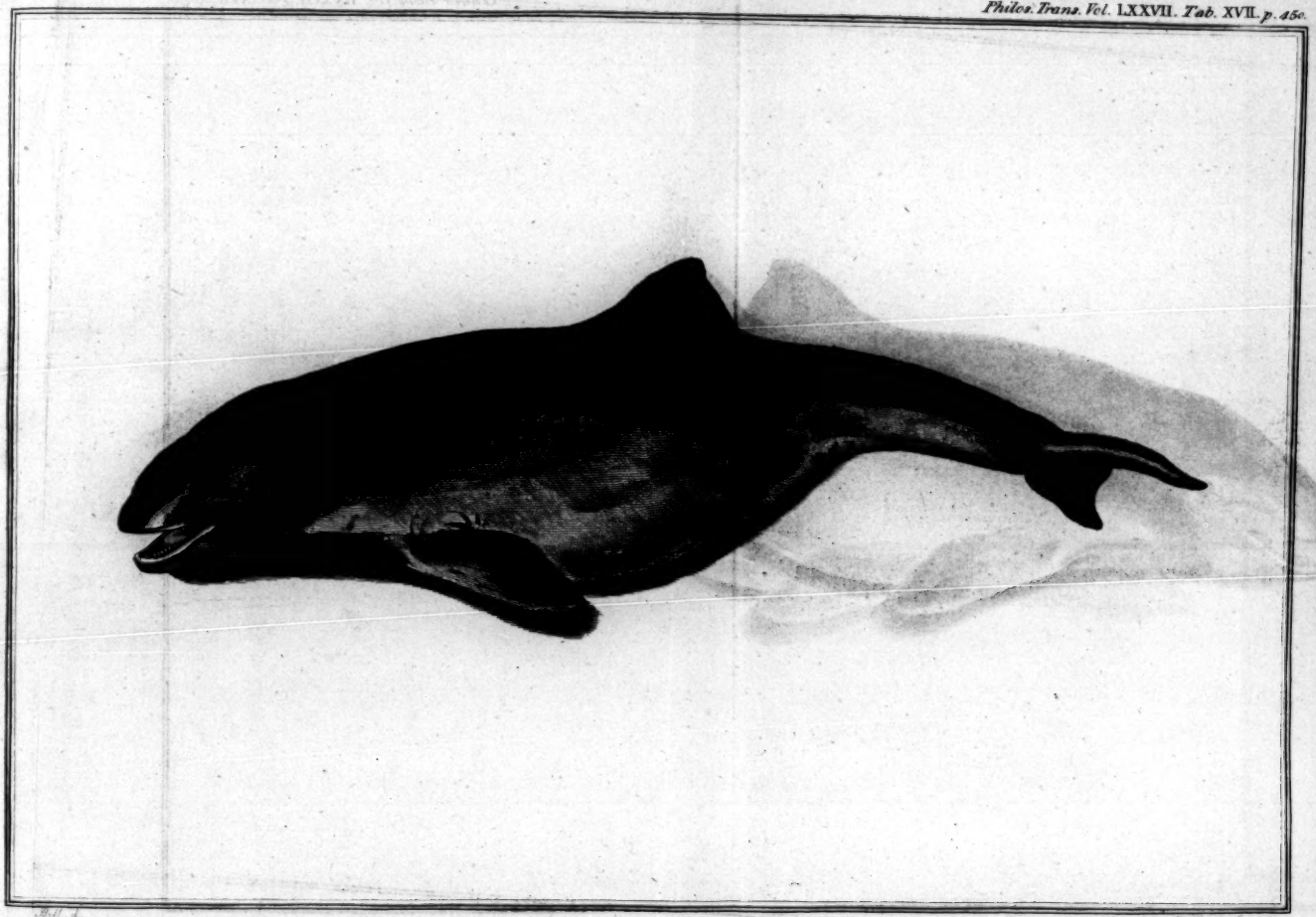
CCCC. The intermediate white ſubſtance, laminæ of which are continued along the middle layer, and form the ſubſtance of the plate of whalebone.

D. The outline of another plate of whalebone.

E. The baſis on which the plates are formed, which adheres to the jaw bone.



Bell. d. *Basire Sc.*



Basire Sc.

Fig. 1.

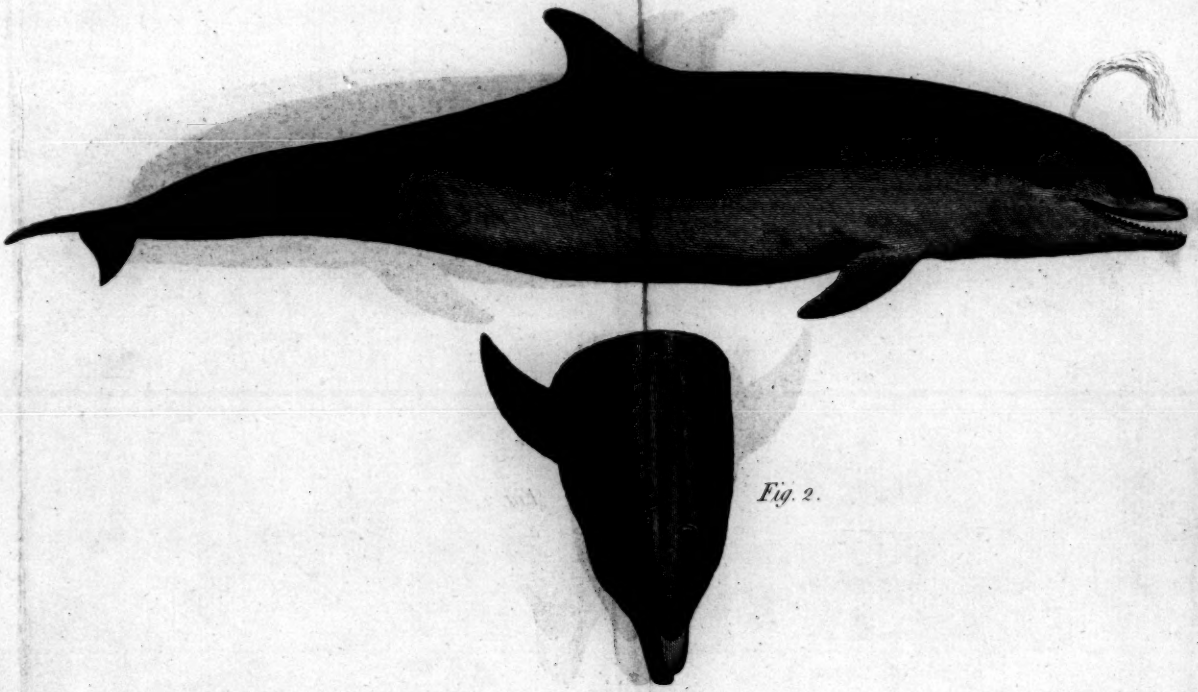
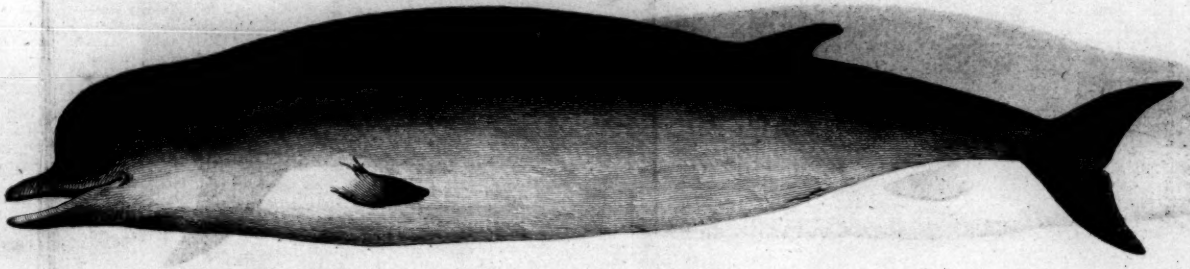


Fig. 2.

Bell. d.

Basire Sc.



	F.	I.
Whole Length	17	0
Upper Jaw from Eye to Eye	1	8
Lower Jaw	2	6
Within the Whalebone	0	10¼
Greatest length of Whalebone	0	5

Fig. 1.

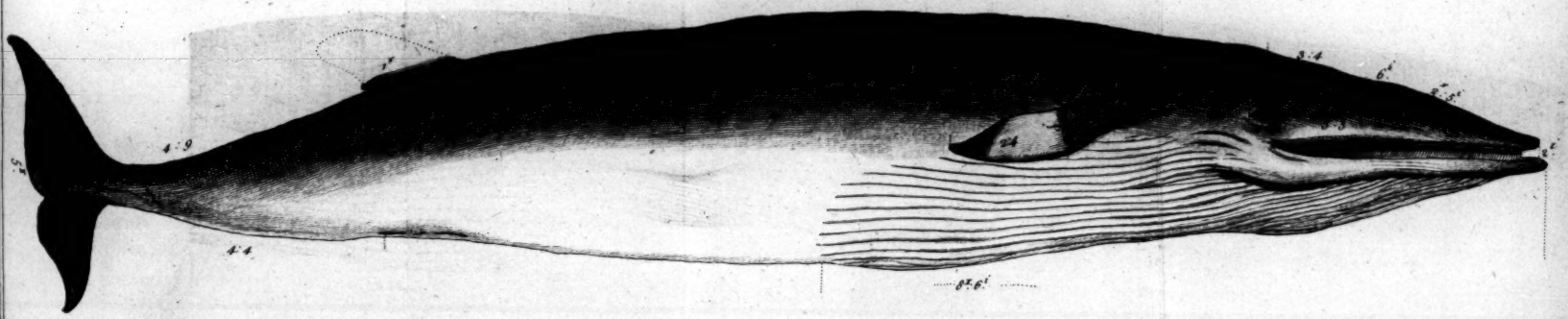
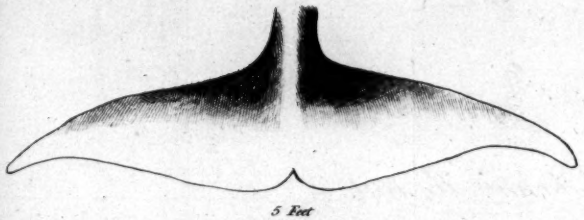


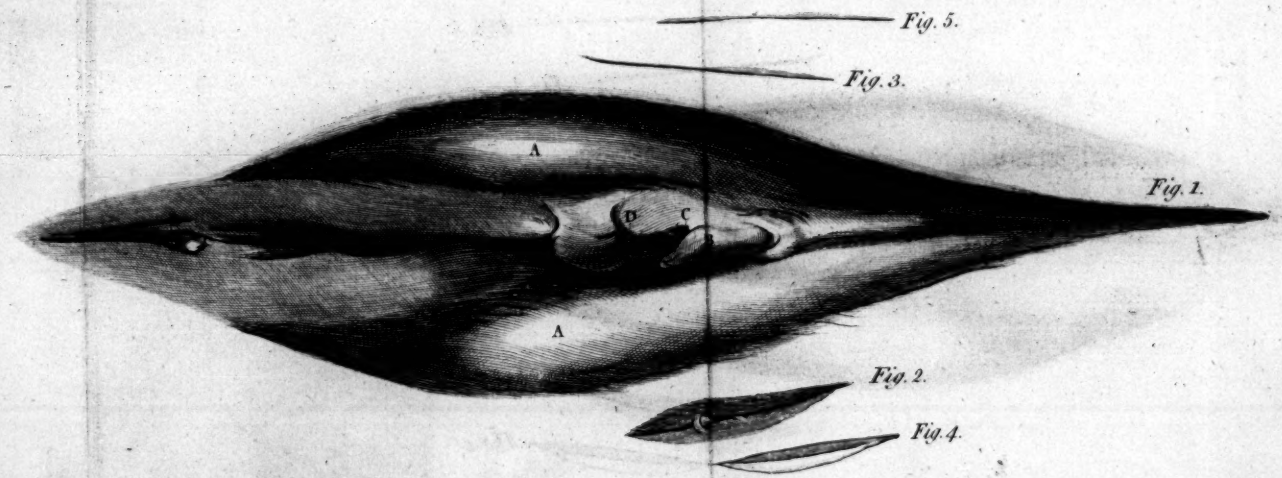
Fig. 2.



Balæna Rostrata.
Fabricius.

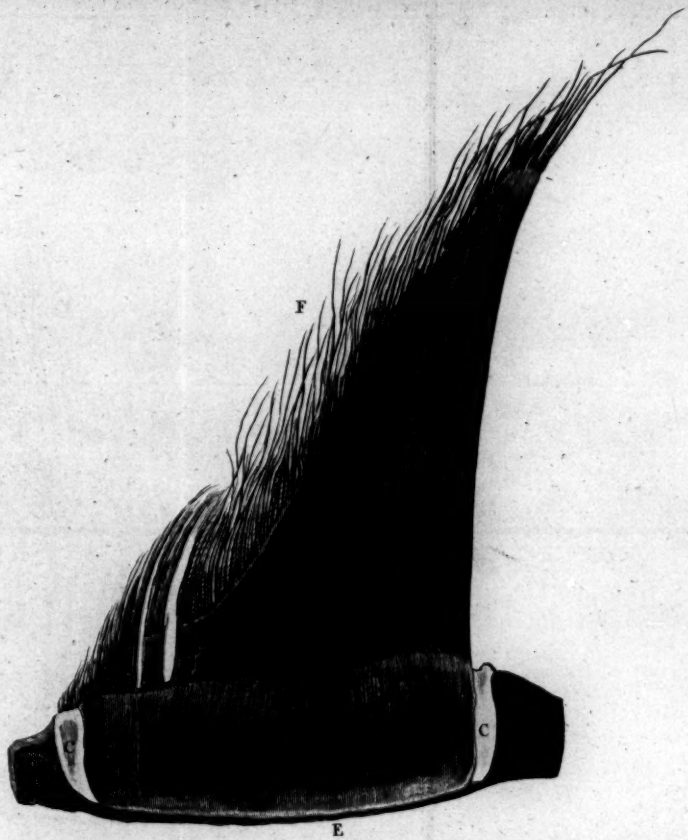
Bell. d.

Bayne Sc.



Bell d.

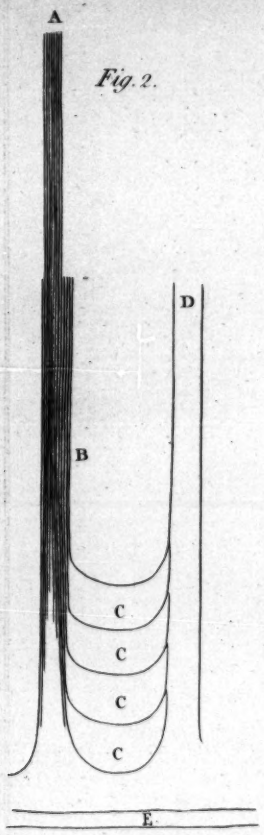
Bafire Sc.



Bell. del. Sharpe. sc.

Fig. 1.

Fig. 2.



Bell d. Sayer sc.

XXXIX. *Some Obſervations on ancient Inks, with the Propoſal of a new Method of recovering the Legibility of decayed Writings.* By Charles Blagden, *M. D. Sec. R. S. and F. A. S.*

Read June 28, 1787.

IN a converſation ſome time ago with my friend Thomas Astle, Eſq. F. R. S. and A. S. relative to the legibility of ancient MSS. a queſtion aroſe, whether the inks in uſe eight or ten centuries ago, and which are often found to have preſerved their colour remarkably well, were made of different materials from thoſe employed in later times, of which many are already become ſo pale as ſcarcely to be read. With a view to the deciſion of this queſtion, Mr. Astle obligingly furniſhed me with ſeveral MSS. on parchment and vellum, from the ninth to the fifteenth centuries inclusively; ſome of which were ſtill very black, and others of different ſhades of colour, from a deep yellowiſh brown to a very pale yellow, in ſome parts ſo faint as to be ſcarcely viſible. On all of theſe I made experiments with the chemical re-agents which appeared to me beſt adapted to the purpoſe; namely, alkalies both ſimple and phlogiſticated, the mineral acids, and infuſion of galls.

It would be tedious and ſuperfluous to enter into a detail of the particular experiments; as all of them, one inſtance only excepted, agreed in the general reſult, to ſhew, that the ink employed anciently, as far as the above-mentioned

MSS. extended, was of the same nature as the present: for the letters turned of a reddish or yellowish brown with alkalies, became pale, and were at length obliterated, with the dilute mineral acids, and the drop of acid liquor which had extracted a letter, changed to a deep blue or green on the addition of a drop of phlogisticated alkali; moreover, the letters acquired a deeper tinge with the infusion of galls, in some cases more, in others less. Hence it is evident, that one of the ingredients was iron, which there is no reason to doubt was joined with the vitriolic acid; and the colour of the more perfect MSS. which in some was a deep black, and in others a purplish black, together with the restitution of that colour, in those which had lost it, by the infusion of galls, sufficiently proved that another of the ingredients was astringent matter, which from history appears to have been that of galls. No trace of a black pigment of any sort was discovered, the drop of acid, which had completely extracted a letter, appearing of an uniform pale serrugineous colour, without an atom of black powder, or other extraneous matter, floating in it.

As to the greater durability of the more ancient inks, it seemed, from what occurred to me in these experiments, to depend very much on a better preparation of the material upon which the writing was made, namely, the parchment or vellum; the blackest letters being generally those which had sunk into it the deepest. Some degree of effervescence was commonly to be perceived when the acids came in contact with the surface of these old vellums. I was led, however, to suspect, that the ancient inks contained a rather less proportion of iron than the more modern; for in general the tinge of colour, produced by the phlogisticated alkali in the acid laid upon them, seemed less deep; which, however, might depend in part upon the

length of time they had been kept: and perhaps more gum was uſed in them, or poſſibly they were waſhed over with ſome kind of varniſh, though not ſuch as gave any gloſs.

One of the ſpecimens ſent me by Mr. Astle proved very different from the reſt. It was ſaid to be a MS. of the fifteenth century; and the letters were thoſe of a full engroſſing hand, angular, without any *fine* ſtrokes, broad, and very black. On this none of the above-mentioned re-agents produced any conſiderable effect; moſt of them rather ſeemed to make the letters blacker, probably by cleaning the ſurface; and the acids, after having been rubbed ſtrongly upon the letters, did not ſtrike any deeper tinge with the phlogiſticated alkali. Nothing had a ſenſible effect toward obliterating theſe letters but what took off part of the ſurface of the vellum; when ſmall rolls, as of a dirty matter, were to be perceived. It is therefore unqueſtionable, that no iron was uſed in this ink; and from its reſiſtance to the chemical ſolvents, as well as a certain clotted appearance in the letters when examined cloſely, and in ſome places a ſlight degree of gloſs, I have little doubt but they were formed with a compoſition of a black ſooty or carbonaceous powder and oil, probably ſomething like our preſent Printer's ink, and am not without ſuſpicion that they were actually printed *.

Whilſt I was conſidering of the experiments to be made, in order to aſcertain the compoſition of ancient inks, it occurred to me, that perhaps one of the beſt methods of reſtoring legibility to decayed writing might be, to join phlogiſticated alkali with the remaining calx of iron; becauſe, as the quantity of pre-

* A ſubſequent examination of a larger portion of this ſuppoſed MS. has ſhewn, that it is really part of a very ancient printed book.

cipitate

cipitate formed by these two substances very much exceeds that of the iron alone, the bulk of colouring matter would thereby be greatly augmented. M. BERGMAN was of opinion, that the blue precipitate contains only between a fifth and a sixth part of its weight of iron; and though subsequent experiments * tend to shew that, in some cases at least, the proportion of iron is much greater, yet upon the whole it is certainly true, that if the iron left by the stroke of a pen were joined to the colouring matter of phlogisticated alkali, the quantity of Prussian blue thence resulting would be much greater than the quantity of black matter originally contained in the ink deposited by the pen; though perhaps the body of colour might not be equally augmented. To bring this idea to the test, I made a few experiments as follows.

The phlogisticated alkali was rubbed upon the bare writing, in different quantities; but in general with little effect. In a few instances, however, it gave a bluish tinge to the letters, and increased their intensity, probably where something of an acid nature had contributed to the diminution of their colour.

Reflecting that when the phlogisticated alkali forms its blue precipitate with iron, the metal is usually first dissolved in an acid, I was next induced to try the effect of adding a dilute mineral acid to writing, besides the alkali. This answered fully to my expectations; the letters changing very speedily to a deep blue colour, of great beauty and intensity. It seems of little consequence as to the strength of colour obtained, whether the writing be first wetted with the acid, and then the phlogisticated alkali be touched upon it, or whether the process be inverted, beginning with the alkali; but on another account, I think, the

* CRELL Beyträge, B. I. st. 1. p. 42, &c.

latter

latter way preferable. For the principal inconvenience which occurs in the propoſed method of reſtoring MSS. is, that the colour frequently ſpreads, and ſo much blots the parchment, as to detract greatly from the legibility; now this appears to happen in a leſs degree when the alkali is put on firſt, and the dilute acid is added upon it. The method I have hitherto found to anſwer beſt has been, to ſpread the alkali thin with a feather over the traces of the letters, and then to touch it gently, as nearly upon or over the letters as can be done, with the diluted acid, by means of a feather, or a bit of ſtick cut to a blunt point. Though the alkali has occaſioned no ſenſible change of colour, yet the moment that the acid comes upon it, every trace of a letter turns at once to a fine blue *, which ſoon acquires its full intenſity, and is beyond compariſon ſtronger than the colour of the original trace had been. If now the corner of a bit of blotting paper be carefully and dexterouſly applied near the letters, ſo as to ſuck up the ſuperfluous liquor, the ſtaining of the parchment may be in great meaſure avoided: for it is this ſuperfluous liquor, which, abſorbing part of the colouring matter from the letters, becomes a dye to whatever it touches. Care muſt be taken not

* The *phlogiſticated alkali* (which is to be conſidered ſimply as a name) appears to conſiſt of a peculiar acid, in the preſent extenſive acceptation of that term, joined to the alkali. Now the theory of the above-mentioned proceſs I take to be, that the mineral acid, by its ſtronger attraction for the alkali, diſlodges the colouring (Pruſſian) acid, which then immediately ſeizes on the calx of iron, and converts it into Pruſſian blue, without moving it from its place. But if the mineral acid be put upon the writing firſt, the calx of iron is partly diſſolved and diffuſed by that liquor before the Pruſſian acid combines with it; whence the edges of the letters are rendered more indiſtinct, and the parchment is more tinged. The ſudden evolution of ſo a fine colour, upon the mere traces of letters, affords an amuſing ſpectacle.

to bring the blotting paper in contact with the letters, becauſe the colouring matter is ſoft whilſt wet, and may eaſily be rubbed off.

The acid I have chiefly employed has been the marine; but both the vitriolic and nitrous ſucceed very well. They ſhould undoubtedly be ſo far diluted as not to be in danger of corroding the parchment, after which the degree of ſtrength does not ſeem to be a matter of much nicety.

The method now commonly practiſed to reſtore old writings, is by wetting them with an infuſion of galls in white wine *. This certainly has a great effect; but it is ſubject, in ſome degree, to the ſame inconvenience as the phlogiſticated alkali, of ſtaining the ſubſtance on which the writing was made. Perhaps if, inſtead of galls themſelves, the peculiar acid or other matter which ſtrikes the black with iron were ſeparated from the ſimple aſtringent matter, for which purpoſe two different proceſſes are given by PIEPENBRING † and by SCHEELE ‡, this inconvenience might be avoided. It is not improbable likewiſe, that a phlogiſticated alkali might be prepared, better ſuited to this object than the common; as by rendering it as free as poſſible from iron, diluting it to a certain degree, or ſubſtituting the volatile alkali for the fixed. Experiment would moſt likely point out many other means of improving the proceſs deſcribed above; but in its preſent ſtate I

* See a complicated proceſs for the preparation of ſuch a liquor in CANEPARIUS, De Atramentis, p. 277.

† CRELL. Annal. 1786, B. I. p. 51.

‡ Kongl. Vetenſk Acad. Nya Handlingar, tom. VII. p. 30. See alſo M. DE MORVEAU's account of this ſubſtance in the Encyclopedie par ordre des matières.

hope it may be of ſome uſe, as it not only brings out a prodigious body of colour upon letters which were before ſo pale as to be almoſt inviſible, but has the further advantages over the infuſion of galls, that it produces its effect immediately, and can be confined to thoſe letters only for which ſuch aſſiſtance is wanted.

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Feb. 1.	Original Letters, written during the Reigns of Henry VI. Edward IV.	John Fenn, Eſq.

and

1787.	Presents.	Donors.
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	Florulæ Insularum Australium prodromus, Auctore G. Forster. Gottingæ, 1786. 8°	George Forster, M.D. F.R.S.
	Poems and Essays, by a Lady lately deceased. The second Edition. Bath, 1786. 2 Vol. 12°	Thomas Bowdler, Esq. F.R.S.
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	An Account of the Effects of Swinging, employed as a Remedy in the pulmonary Consumption. London, 1787. 8°	James Carmichael Smyth, M.D. F.R.S.
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	Friedrichs Sternendenkmal, vom Herrn Bode. Berlin, 1787. 4°	Mr. J. E. Bode, Astron. to the Academy at Berlin.
	A. J. Testa de Vitalibus Periodis Ægrotantium et Sanorum. 2 Vol. Londini, 1787. 8°	Professor Testa of Ferrara.
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1787.	
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Nuove Esperienze intorno alla dolcificazione dell' acqua del Mare, del Sig. Cav. Lorgna. 4°	Cavaliere Lorgna.
Tertia Dissertatio Botanica, Auctore A. J. Cavanilles. Parisiis, 1787. 4°	Abbé Cavanilles.
De Maximis et Minimis, Pars prior, Auctore Sim. Lhuilier. Varsaviæ, 1782. 4°	M. Lhuilier.
April 19. Expériences sur les Végétaux, par J. Ingenhousz. Paris, 1787. 8°	John Ingen-housz, M. D. F.R.S.
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A. J. Testa de re Medica et Chirurgica, Epistolæ VII. Ferrariæ, 1781. 8°	———
26. Kongl. Vetenskaps Academiens nya Handlingar. 2d Quarter of Tom. VII. for 1786. 8°	The Royal Academy of Sciences of Stockholm.
Première Continuation des Expériences faites par le moyen de la Machine Electrique Teylerienne, par M. van Marum. Harlem, 1787. 4°	Martin van Marum, M.D.
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1787.	Presents.	Donors.
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	The one great Argument for the Truth of Christianity from a single Prophecy, evinced in a new Explanation of the Seventh Chapter of Isaiah. Yarmouth, 1787. 8°	The Rev. Samuel Cooper, D.D.
June 7.	Mémoires de l'Académie Royale des Sciences, Année 1784. Paris, 1787. 4°	The Royal Academy of Sciences of Paris.
	Rapport des Commissaires chargés par l'Académie de l'Examen du Projet d'un nouvel Hotel-Dieu. Paris, 1780. 4°	
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	Idées sur la Météorologie, par J. A. De Luc, Tome II. Londres, 1787. 8°	M. De Luc, F.R.S.
	The Elevation, Section, Plan, and Views of a Triple Vessel and of Wheels, by P. Miller. Edinburgh, 1787. fol.	Patrick Miller, Esq.
14.	Medical Remarks on natural, spontaneous, and artificial Evacuation, by J. Anderson. London, 1787. 8°	John Anderson, M.D.
21.	Tableau des Variétés de la Vie Humaine, par M. G. Daignan. Paris, 1786. 2 Vol. 8°	M. Daignan.
28.	Meterological Journal kept at Odiham, in Hampshire, from April 1, 1786, to April 1, 1787, by A. Baxter, MS. fol.	Alexander Baxter, Esq.
	Meteorological Journal kept at Bencoolen, from Sept. 1. to Nov. 30, 1784, by John Macdonald, MS. 4°	Capt. Macdonald.

APPENDIX.

A Supplement to Major-General Roy's *Account of the Mode propoſed to be followed in determining the relative Situation of the Royal Obſervatories of* Greenwich *and* Paris. See p. 188.

Read Nov. 8, 1787.

IN the account of the propoſed trigonometrical operation for determining the difference between the meridians of the Royal Obſervatories of Greenwich and Paris, I have, at p. 221. and 222. had occaſion to remark on an inconſiſtency found in the ſum of the three equations, as ſtated by M. Bouguer, for obtaining the lengths of degrees of great circles perpendicular to the meridian, above their correſponding degrees of latitude, without having been aware of the true ſource of the error, for which I am indebted to the inveſtigations of the Aſtronomer Royal; who, having found it out, obligingly communicated the ſame to me, about the time of the annual receſs of the Society in the end of June laſt.

At p. 289. and again at p. 313. and 314. of M. Bouguer's Book, the ſubtractive branch of the equation, or the part of DG the gravicentric arc, anſwering to the difference between the *radii* of curvature at the equator and given latitude, has erroneouſly been expreſſed in words $\frac{1}{3}$ths inſtead of $\frac{1}{4}$ths, which the algebraic *formula* juſtly gives for it, and according to which M. Bouguer's table has been accurately computed. Not ſuſpecting any thing of this ſort, no intima-

tion of it appearing among the *errata*, and no notice having been hitherto taken, as far as I know, of such a mistake existing in that justly celebrated work, now so many years in circulation throughout the world; instead therefore of $\frac{4}{17}$ths, or $\frac{240}{1020}$ parts of the third proportional, the last additive member of the equation, I substituted another (with a certain modification, however, as stated in the before-mentioned pages) amounting only to $\frac{210}{1770}$ parts of the third proportional, being the highest that would apply to the whole quadrant, without producing absurd results. Thus I obtained approximate degrees of great circles and of longitude, differing but little from those of M. Bouguer, and compensating in a great measure, although not altogether, for the then undiscovered cause of the mistake of $\frac{1}{240}$th part of the arc DG; for $\frac{4}{17} - \frac{3}{8} = \frac{1}{240}$.

In this state of the case, I have judged it incumbent on me to annex a supplementary table, where the degrees of great circles and of longitude are accurately computed by the corrected subtractive branch $\frac{4}{17}$ths of DG instead of $\frac{3}{8}$ths, as it now stands in the original table. From inspection it will appear, that the maximum of correction amounts nearly to $5\frac{1}{4}$ fathoms at the 70th degree of latitude, diminishing gradually from thence to the pole on one side, and the equator on the other, where it vanishes. The maximum of correction for the degrees of longitude, amounting to about $2\frac{1}{4}$ fathoms, is applicable between the 58th and 59th degree of latitude, where M. Bouguer's degree of the meridian becomes equal to his degree of longitude on the equator. From this point, it diminishes gradually to the pole on one side, and the equator on the other, where it in like manner disappears.

With regard to degrees of great circles situated obliquely to the meridian, it is sufficiently obvious, that they are so little affected as to render it of but small importance whether they

are corrected or not; but for such as are scrupulous to fractional parts of fathoms, these may satisfy themselves with great facility and exactness, by attending to and proportioning by the common tabular differences, as in the following example of the application of the correction to oblique degrees, in the latitude of Greenwich 51° 28′ 40″.

	Obliquity.	Original tabular difference.	Supp. correction.	Corrected difference.	Corrected degree.
	° ′	Fath.	Fath.	Fath.	Fath.
Application of the correction to the degrees of great circles, situated obliquely to the meridian, in the latitude of Greenwich.	45 0	204.45	−1.65	+ 202.80	61070.20
	33 45 / 56 15	78.24	−0.63	− 77.61	60992.63 / 61147.85
	22 30 / 67 30	66.33	−0.54	− 65.79	60926.84 / 61213.64
	11 15 / 78 45	44.32	−0.36	− 43.96	60882.88 / 61257.60
	0 0 / 90 0	15.56	−0.12	− 15.44	60867.44 / 61273.04

Here it is to observed, that half of the correction is constantly to be applied at the 45th degree of obliquity; and since the differences between the terms in the progression equally removed on either side from 45°, are always equal to each other, it follows, that the corrected differences are to be applied with the contrary sign, those between 45° and the meridian being in diminution, and these between 45° and the east or west points being in augmentation of the length, till, in the first case, the degree becomes equal to that of latitude; and in the last to that of a great circle perpendicular to the meridian.

The Table of Comparison, p. 227. is no where affected by the alteration which has been the subject of this discussion, except in the two last lines from the bottom, as adverted to among the *errata* subjoined to this supplement.

This intimation of M. Bouguer's mistake in expressing his formula in words, was accompanied with the conversion of that

that formula by Dr. MASKELYNE into the following, adapted to natural lines, $\frac{a \times 11 + 8 \times \text{cos. } 2 \text{ lat} - 3 \text{ cos. } 4 \text{ lat.}}{30}$. a is the excess of the radius of curvature at the pole above that at the equator, and the sign of either term is only to be changed when the doubled or quadrupled latitude become greater than 90°, and less than 270°.

I embrace this opportunity of mentioning another circumstance, wholly unknown to me at the time my Paper was composed. From what has been there said, at p. 216. and so on to p. 220. it will probably be inferred, that I considered the proposed mode of determining the differences of longitude by the observations of the pole star, made with a very accurate instrument, rather as new, not knowing that the same idea, or one nearly the same, had before occurred to the Rev. Mr. MICHELL, and been treated on by him in his very ingenious Paper in the Philosophical Transactions, Vol. LVI. for the year 1766. That I must have read that valuable performance about the time of its publication is not to be doubted; but in the lapse of so many years, every trace of it had gone from my remembrance, otherwise I would have most certainly referred to it in the proper place, and with the attention that it so well deserves. However, without entering here into particulars, it will obviously appear, that the one has not been borrowed from the other.

ERRATA.

In p. 195. l. 27. for between Gravelines and Calais read between Watten and Gravelines. And in l. 28. for 56° 42′ 0″ read 46° 52′ 0″.

In p. 197. in the distance of the parallel of Rodes from that of Dunkirk, for 6° 50′ 51″ 14‴ read 6′ 40″ 51″ 14‴.

In the last line of the note at the bottom of p. 217. instead of 1° 49′ 8″.8, read 1° 49′ 48″.8.

In p. 221. line 6. from the top, instead of $\frac{1}{75}$ths, read $\frac{1}{17}$ths.

In the Table of Comparison, p. 227. the last line but one of the last column but one, for 44373.5 read 44372.0. In the next line below, for 38164.0, as formerly corrected, read 38162.0. And in the right-hand column, last line but one from the bottom, for +19.1, read +17.6.

Places.	Lanitudes.	Sum of the 3 equat. ⅗ths of DG being subtractive.	Supplementary correction for deg. of great circles.	Degrees of great circles perpendicular to the meridian corrected.	Diff.	Supplementary correction for deg. of long.	Degrees of longitude corrected.	Diff.
Equator	° ′ ″ 0 0 0	545.12	±0.00	61029.62		0.00	61029.62	
	5 — —	547.081	+0.07	61031.64	2.02	+0.04	60799.38	230.24
	10 — —	552.595	+0.82	61038.02	6.38	+0.86	60110.72	688.66
	15 — —	558.739	−0.90	61047.82	9.80	−0.85	58967.68	1143.04
	20 — —	565.811	−0.70	61064.30	16.48	−0.67	57381.66	1586.02
	25 — —	567.715	−0.83	61084.82	20.52	−0.74	55361.65	2020.01
	30 — —	562.156	−0.99	61110.54	25.72	−0.86	52923.27	2438.38
	35 — —	546.285	−1.25	61141.42	30.88	−1.02	50084.11	2839.16
	40 — —	518.144	−1.73	61177.13	35 71	−1.33	46864.41	3219.70
					7.68			687.63
	41 — —	510.955	−1.78	61184.81		−1.34	46176.78	
	42 — —	503.235	−1.90	61192.64	7.83	−1.40	45475.00	701.78
	43 — —	494.946	−2.07	61200.57	7.93	−1.51	44759.26	715.74
S. of France	43 32 —	490.392	−2.11	61204.93	4.36	−1.53	44371.97	387.29
	44 — —	486.244	−2.16	61208.74	3.81	−1.54	44029.89	342.08
	45 — —	476.984	−2.30	61217.00	8.26	−1.66	43286.92	742.97
	46 — —	467.216	−2.42	61225.39	8.39	−1.67	42530.74	756.18
	47 — —	456.972	−2.58	61233.89	8.50	−1.77	41761.40	769.34
	48 — —	446.252	−2.75	61242.49	8.60	−1.83	40979.23	782.27
Paris	48 50 14	436.936	−2.83	61249.77	7.28	−1.87	40314.64	664.59
	49 — —	435.084	−2.85	61251.18	1.41	−1.87	40184.39	130.25
	50 — —	423.488	−3.03	61259.96	8.78	−1.94	39377.15	807.24
					0.73			67.83
	50 5 —	422.504	−3.06	61260.69		−1.97	39309.32	
	50 10 —	421.508	−3.06	61261.43	0.74	−1.97	39241.42	67.90
	50 15 —	420.523	−3.07	61262.16	0.73	−1 95	39173.42	68.00
	50 20 —	419.534	−3.11	61262.90	0.74	−1.99	39105.33	68.09
	50 25 —	418.526	−3.13	61263.64	0.74	−1.99	39037.19	68.14
	50 30 —	417.531	−3.14	61264.37	0.73	−2.01	38968 93	68.26
	50 35 —	416.528	−3.15	61265.11	0.74	−2.02	38900.60	68.33
	50 40 —	415.524	−3.16	61265.84	0.73	−2.03	38832.19	68.41
	50 45 —	414.515	−3.20	61266.59	0.75	−2.03	38763.69	68 50
	50 50 —	413.500	−3.21	61267.32	0.73	−2.04	38695.11	68.58
	50 55 —	412.491	−3.21	61268.06	0.74	−2.02	38626.46	68.65
	51 0 0	411.477	−3.22	61268.80	0.74	−2.01	38557.71	68.75

e for the Degrees of the Earth.

Places.	Latitudes.	Sum of the 3 equat. ⅓ths of DG being subtractive.	Supplementary correction for deg. of great circles.	Degrees of great circles perpendicular to the meridian corrected.	Diff.	Supplementary correction for deg. of long.	Degrees of longitude corrected.	Diff.
	° ′ ″							
	51 0 0	411.477	−3.22	61268.80		−2.01	38557.71	
					0.74			68.83
	51 5 —	410.459	−3.22	61269.54		−2.02	38488.88	
					0.74			68.91
	51 10 —	409.437	−3.25	61270.28		−2.03	38419.97	
					0.74			69.00
	51 15 —	408.408	−3.29	61271.02		−2.06	38350.97	
					0.74			69.07
	51 20 —	407.387	−3.29	61271.76		−2.07	38281.90	
					0.74			69.17
	51 25 —	406.357	−3.30	61272.50		−2.09	38212.73	
Greenwich	51 28 40	405.601	−3.30	61273.04	0.54	−2.04	38161.96	50.77
					0.20			18.48
	51 30 —	405.320	−3.31	61273.24		−2.09	38143.48	
					0.74			69.31
	51 35 —	404.291	−3.32	61273.98		−2.07	38074.17	
					0.74			69.41
	51 40 —	403.253	−3.32	61274.72		−2.07	38004.76	
					0.74			69.48
	51 45 —	402.213	−3.37	61275.46		−2.06	37935.28	
					0.75			69.59
	51 50 —	401.168	−3.37	61276.21		−2.08	37865.69	
					0.74			69.68
	51 55 —	400.128	−3.38	61276.95		−2.11	37796.01	
					0.74			69.70
	52 0 0	399.080	−3.40	61277.69		−2.09	37726.31	
					8.94			843.09
	53 — —	386.325	−3.35	61286.63		−2.13	36883.22	
					8.95			854.59
	54 — —	373.232	−3.73	61295.58		−2.20	36028.63	
					8.97			865.79
	55 — —	359.851	−3.90	61304.55		−2.24	35162.84	
					8.97			876.75
	56 — —	346.187	−4.07	61313.52		−2.27	34286.09	
					8.94			887.48
	57 — —	332.301	−4.23	61322.46		−2.30	33398.61	
					8.91			897.94
	58 — —	318.213	−4.39	61331.37		−2.33	32500.67	
					6.33			646.60
	58 42 47·4	308.075	−4.51	61337.70		−2.35	31854.07	
					2.53			261.52
	59 — —	303.963	−4.55	61340.23		−2.35	31592.55	
					8.80			918.03
	60 — —	289.569	−4.69	61349.03		−2.34	30674.52	
					42.39			4729.38
	65 — —	217.320	−5.28	61391.42		−2.23	25945.14	
					10.69			1297.41
Lapland	66 20 —	198.361	−5.39	61402.11		−2.16	24647.73	
					27.58			3637.54
	70 — —	148.229	−5.47	61429.69		−1.87	21010.19	
					32.38			5102.64
	75 — —	87.821	−4.89	61462.07		−1.27	15907.55	
					25.39			5230.44
	80 — —	41.569	−3.81	61487.46		−0.74	10677.11	
					13.98			5316.91
	85 — —	10.309	−2.16	61501.44		−0.20	5360.20	
Pole	90 0 0	0.000	∓0.00	61506.60	5.16	∓0.00	0000.00	5360.20

N. B. The degree of the meridian, whose center answers to the latitude of 43° 32′, is in length 60714.54 fathoms.

Supplementary

Place.	Latitudes.	Sun's distance from the equator.	Diff. between simple and equation of time.	Diff. between simple and mean motion.	Degrees of great circle perpendicular to the meridian corresponding to	Sulphurous acid in the air.	Degrees of humidity of the air.
	° ′ ″	° ′ ″	″	″			
Alexandria	31 13 0 N.	23 50 0 N.	0.00	0.00	0.00	0.00	0.00
	30 — —	23 41 30 —	+ 0.01	+ 0.04	0.04	+ 0.04	0.00
	25 — —	23 13 0 —	+ 0.05	+ 0.16	0.16	+ 0.20	0.00
	20 — —	22 34 0 —	+ 0.11	+ 0.36	0.36	+ 0.56	0.00
	15 — —	21 45 0 —	+ 0.20	+ 0.64	0.64	+ 1.20	0.00
	10 — —	20 46 0 —	+ 0.31	+ 1.00	1.00	+ 2.20	0.00
	5 — —	19 37 0 —	+ 0.44	+ 1.44	1.44	+ 3.64	0.00
	0 — —	18 18 0 —	+ 0.60	+ 1.96	1.96	+ 5.60	0.00
	5 — —	16 50 0 —	+ 0.78	+ 2.56	2.56	+ 8.16	0.00
	10 — —	15 13 0 —	+ 0.99	+ 3.24	3.24	+ 11.40	0.00
	15 — —	13 27 0 —	+ 1.22	+ 4.00	4.00	+ 15.40	0.00
	20 — —	11 32 0 —	+ 1.48	+ 4.84	4.84	+ 20.24	0.00
	25 — —	9 29 0 —	+ 1.76	+ 5.76	5.76	+ 26.00	0.00
	30 — —	7 18 0 —	+ 2.07	+ 6.76	6.76	+ 32.76	0.00
	35 — —	4 59 0 —	+ 2.40	+ 7.84	7.84	+ 40.60	0.00
	40 — —	2 32 0 —	+ 2.76	+ 9.00	9.00	+ 49.60	0.00
	45 — —	0 0 0 —	+ 3.15	+ 10.24	10.24	+ 59.84	0.00
Cape of Good Hope	35 — —	4 59 0 S.	+ 3.57	+ 11.56	11.56	+ 71.40	0.00
	40 — —	2 32 0 —	+ 4.01	+ 12.96	12.96	+ 84.36	0.00
	45 — —	0 0 0 —	+ 4.48	+ 14.44	14.44	+ 98.80	0.00
	50 — —	2 32 0 N.	+ 4.97	+ 16.00	16.00	+ 114.80	0.00
	55 — —	5 4 0 —	+ 5.49	+ 17.64	17.64	+ 132.44	0.00
	60 — —	7 47 0 —	+ 6.03	+ 19.36	19.36	+ 151.80	0.00
	65 — —	10 40 0 —	+ 6.60	+ 21.16	21.16	+ 172.96	0.00
	70 — —	13 43 0 —	+ 7.19	+ 23.04	23.04	+ 196.00	0.00
	75 — —	16 56 0 —	+ 7.81	+ 25.00	25.00	+ 221.00	0.00
	80 — —	20 19 0 —	+ 8.45	+ 27.04	27.04	+ 248.04	0.00
	85 — —	23 52 0 —	+ 9.12	+ 29.16	29.16	+ 277.20	0.00
	90 — —	27 36 0 —	+ 9.81	+ 31.36	31.36	+ 308.56	0.00
Paris	48 50 14	23 50 0 N.	+ 2.80	+ 9.00	9.00	+ 49.60	0.00
	50 — —	24 35 0 —	+ 2.85	+ 9.16	9.16	+ 58.76	0.00
	55 — —	26 52 0 —	+ 3.02	+ 9.72	9.72	+ 68.48	0.00
	60 — —	29 23 0 —	+ 3.06	+ 9.84	9.84	+ 78.32	0.00
	65 — —	31 58 0 —	+ 3.00	+ 9.64	9.64	+ 87.96	0.00
	70 — —	34 26 0 —	+ 2.84	+ 9.12	9.12	+ 97.08	0.00
	75 — —	36 37 0 —	+ 2.59	+ 8.32	8.32	+ 105.40	0.00
	80 — —	38 22 0 —	+ 2.25	+ 7.24	7.24	+ 112.64	0.00
	85 — —	39 34 0 —	+ 1.83	+ 5.88	5.88	+ 118.52	0.00
	90 — —	40 13 0 —	+ 1.34	+ 4.32	4.32	+ 122.84	0.00
	95 — —	40 22 0 —	+ 0.77	+ 2.48	2.48	+ 125.32	0.00
	100 — —	40 0 0 —	+ 0.13	+ 0.40	0.40	+ 125.72	0.00
	105 — —	38 52 0 —	+ 0.58	+ 1.88	1.88	+ 123.84	0.00
	110 — —	37 13 0 —	+ 1.19	+ 3.84	3.84	+ 120.00	0.00
	115 — —	35 0 0 —	+ 1.77	+ 5.72	5.72	+ 114.28	0.00
	120 — —	32 23 0 —	+ 2.30	+ 7.40	7.40	+ 106.88	0.00
	125 — —	29 23 0 —	+ 2.78	+ 8.96	8.96	+ 97.92	0.00
	130 — —	26 0 0 —	+ 3.20	+ 10.32	10.32	+ 87.60	0.00
	135 — —	22 16 0 —	+ 3.56	+ 11.48	11.48	+ 76.12	0.00
	140 — —	18 11 0 —	+ 3.86	+ 12.44	12.44	+ 63.68	0.00
	145 — —	13 47 0 —	+ 4.09	+ 13.20	13.20	+ 50.48	0.00
	150 — —	9 4 0 —	+ 4.26	+ 13.72	13.72	+ 36.76	0.00
	155 — —	4 4 0 —	+ 4.37	+ 14.08	14.08	+ 22.68	0.00
	160 — —	0 0 0 —	+ 4.41	+ 14.20	14.20	+ 8.48	0.00

Tranſlation of Father Joſeph da Rovato's *Letter to the* Royal Society, *relative to Borax.* See p. 301.

THE Father prefect of the Miſſion in Thibet has the pleaſure to acquaint the Royal Society, that, reſiding at Patna, he has frequently been deſired by M. VOGLES, an able naturaliſt from Germany, to obtain ſome circumſtantial account of the places where, and the manner in which, the borax procured from the kingdom of Thibet is obtained; no one elſe, as he ſaid, having any communication with thoſe almoſt impenetrable parts. Although our Miſſion have long ſince forſaken that kingdom, yet the Father prefect being ſomewhat connected with the *Bahadur Shah*, brother to the King of Nepal (whoſe kingdom extends northward as far as Kuti on the frontiers of Thibet), he wrote to him, and requeſted all the information that could be obtained on the ſubject. The Bahadur Shah, in order to give the beſt ſatisfaction in his power, was pleaſed to ſend to the prefect, as far as Patna, a man in his ſervice, who, being a native of the country where the Borax is prepared, could give the moſt ample intelligence concerning that ſubſtance.

This man, partly in the Nepaleſe and partly in the Hindoo language, both which are underſtood by the prefect, gave the following account. In the province or territory of Marmé, twenty-eight days journey to the north of Nepal, and twenty-five to the Weſt

of Laſſa, the capital of Thibet, there is a vale about eight miles broad. In a part of this vale there are two villages or caſtles, the one named *Scierugh*, and the other *Kanglé*, the inhabitants of which are wholly employed in digging the borax, which they ſell into Thibet and Nepal, they having no other means of ſubſiſtence, the ſoil being ſo barren as to produce nothing but a few ruſhes. Near the two above-mentioned caſtles there is a pool of a moderate ſize, and ſome ſmaller ones, where the ground is hollow, in which the rain-water collects. In theſe pools, after the water has been ſome time detained in them, the borax is formed naturally: the men, wading into the water, feel a kind of a pavement under their feet, which is a ſure indication that borax is there formed, and there they accordingly dig it.

Where there is little water, the layer of borax is thin; and where it is deep, it is thicker, and over the latter there is always an inch or two of ſoft mud, which is probably a depoſit of the water, after it has been agitated by rain or wind. Thus is the borax produced merely by nature, without either boiling or diſtillation. The water in which it is formed is ſo bad, that the drinking a ſmall quantity of it will occaſion a ſwelling of the abdomen, and in a ſhort time death itſelf. The earth that yields the borax is of a whitiſh colour; and in the ſame valley, about four miles from the pools, there are mines of ſalt, which is there dug in great abundance for the uſe of all the inhabitants of theſe mountains who live at a diſtance from the ſea. The natives, who have no other ſubſiſtence on account of the ſterility of the ſoil, pay nothing for digging borax; but ſtrangers muſt pay a certain retribution, and uſually agree at ſo much a workman. This is paid to a Lama, named Pema *Tupkan*, who owns the pits in Marmé.

Ten

Ten days journey farther north, there is another valley named *Tapré*, where they dig borax, and another ſtill farther, called *Cioga*; but of this latter I have not marked the ſituation. Borax is in the Hindoo and Nepaleſe languages called *Soaga*. If it be not purified, it will eaſily deliqueſce; and in order to preſerve it any time, till they have an opportunity of ſelling it, the people often mix it with earth and butter.

In the territory of *Mungdan*, ſixteen days journey to the north of Nepal, there are rich mines of arſenic; and in various other places are found mines of ſulphur, as alſo of gold and ſilver, whoſe produce is much purer than thoſe of the mines of Pegu.

This is the ſubſtance of the information obtained from the man ſent by the Bahadur Shah. If the Gentlemen of the Royal Society wiſh to ſee any of the ſoil which yields the borax, it may be eaſily obtained, ſince the ſaid Bahadur Shah, who now governs the whole of the province of Nepal, is well diſpoſed towards the Father prefect, and will probably not refuſe him the favour of ſending a truſty perſon to gather ſome of the ſoil, and to ſend it down to Patna. The Father prefect will eaſily find an opportunity of ſending it thence to London.

This is what the Father prefect takes the liberty to mention to the Royal Society. He, moreover, tenders his own beſt ſervices, and thoſe of the other Italian Capuchins, his brethren Miſſionaries, if they could communicate any other uſeful intelligence; they being very deſirous to prove their gratitude to the Engliſh nation, from whom they have received, and are ever receiving, many and ſingular benefits.

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249.	10.	*for* " laſt to the" *read* " laſt term to ten times the"
254.	25.	*for* " has" *read* " have"
301.	13.	*for* " alto" *read* " altro"
302.	9.	*for* " alto" *read* " altro"
305.	18.	*for* " celle" *read* " celles"
——	20.	*for* " at" *read* " et"
——	22.	*for* " cet" *read* " cette"
330.	20.	*for* " was" *read* " were"
336.	23.	*for* " LC" *read* " L$_{\mathfrak{C}}$"
339.	14.	*for* " 2d" *read* " 23d"
——	laſt line.	*for* " 15 13 52.4" *read* " 16 13 52.4"
340.	8.	*for* " 15 h. 13′ 52″.4" *read* " 16 h. 13′ 52″.4
416.	24. and 25.	*for* " we do know" *read* " we do not know"
429.	22.	*for* a full ſtop after " ſenſe" *put* a comma